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THE PSYCHOLOGICAL REVIEW

CONTRIBUTIONS OF FREUDISM TO PSYCHOLOGY

I. INFLUENCE OF FREUDISM ON THEORETICAL PSYCHOLOGY

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It is my purpose to show some of the effects that psychoanalytic writers have had on theoretical psychology. In this discussion I shall group together the psychoanalytic and the related schools of psychology and psychiatry which are known collectively as the 'new' psychology. At different times different meanings have been given to the term 'new psychology.' I shall use the term in its present and current meaning.

If as we read the literature of the new psychology, we stop to recall our psychological reading of only a few years ago, we find that the two kinds of psychology do not even use the same language. The terminology is entirely different. In the psychology which has become established in the colleges and written in textbooks, the discussion is concerned mainly with such categories as sensation and perception, imagination, reasoning, the sense organs, memory, the affective states, and so on. These terms have a familiar sound to any one who has ever taken a course in psychology in a college or a normal school. In the new psychology we read about complexes, rationalization, projection, compensation, identification, symbolism, repression, the wish, and many other categories that do not even occur in the indexes of standard textbooks of the subject. What are the fundamental reasons

for the disparity between the established type of psychological discourse and the so-called new psychology?[1]

There are several factors that contribute toward this disparity. First we must recall the different origins of the old and of the new psychology. The old psychology was written first by philosophers and later by psychologists who devoted themselves to the scientific study of mind. The new psychology, and particularly psychoanalysis, has been developed by those physicians who have devoted themselves primarily to the treatment of mental disorder. Here we have two different types of training for the men who represent the two different types of psychology.

Another factor that partly explains the difference in terminology between the new psychology and the old is to be found in the character of the mental phenomena that are the bases of the two schools. The psychiatrist deals with minds that are abnormal, minds that have broken under distress of some kind. The psychologist deals with normal minds, minds that are sufficiently calm, quiet, and contented to submit to experimentation in the psychological laboratory. Obviously the material on which the two schools of psychology are built up differs at the very source of the observations.

The normal person who has sufficient leisure to serve as a subject of experimentation in the psychological laboratory is not likely to have any major mental disturbance or distress. If, on the occasion of a psychological experiment, he is mentally disturbed by any serious issue in his fundamental life interests, the financial, sexual, social, professional, or physical, he reports that he is indisposed, and he does not serve as a subject. It is, therefore, relatively seldom that the college psychological laboratory gets for observation persons who are in an abnormal mental condition of major significance. The psychiatrist, on the other hand, continually observes persons whose mental states are dominated, or broken, by issues that are close to the fundamental mainsprings of life.

[1] Sections of this paper are quoted from 'The Nature of Intelligence' (in press) Thurstone, The International Library of Psychology and Philosophy, Kegan Paul, French, Truber & Co., London.

It is only reasonable, therefore, that we should find a fundamental difference between the new and the old psychology as regards the significance of the mental phenomena that they represent. The established forms of psychological discussion relate mostly to the *momentary mental states* and related phenomena such as sense qualities, color mixture, taste buds, visual illusions, reflexes, fields of attention, visual and auditory imagery, the momentary nature of emotion, and the differences between instinctive and habitual actions. All of these, and in fact most of the discussions in the standard textbooks of psychology, refer to the momentary mental states, situations in which a laboratory experiment may be prepared and in which the subject reports what he at that moment sees, or hears, or feels. There is no criticism to be made against all such scientific experimentation except that it seldom relates to *the permanent life interests* of the persons who lend their minds to the psychological experiments.

In the new psychology we deal, on the other hand, with a whole series of explanatory categories that have their origin in the psychopathic hospitals where every person observed is giving vent to a disturbance in the fundamental and permanent mainsprings of his life. This contrast between the new psychology and the old is summarized by noting that the established forms of psychological writing deal mostly with the *momentary mental states*, while the new psychology deals mostly with the *expression of basic and permanent human wants*.

In general, a fair-minded student would probably admit that the new psychology deals with subjects that are more generally interesting than those which he may recall from his college textbooks. On the other hand, one must admit also that the psychology of the standard textbooks is written with greater regard for scientific consistency. The new psychology has very little regard for scientific method, and it does not rest on careful experimental work. Nevertheless, the new psychology has a strong appeal to our interests, and in large part its propositions seem to be very plausible. It is only natural that the physicians who devote themselves

primarily to the treatment of disordered minds should pay most attention to the methods that are effective in practice, while the psychologists, as scientists, should pay most attention to the scientific experimental methods for establishing facts.

One of the basic differences between the old and the new psychology is in the treatment of the stimulus or environment. Writers of the scientific schools of psychology treat the stimulus as the datum for psychological inquiry. They put their subject into the laboratory and confront him with stimuli of various kinds—colors, noises, pains, words; and with the stimulus as a starting point they note what happens. The behavior of the person is interpreted largely as a function of the stimulus or environment. The person's own inclinations are, of course, recognized as constituting a factor in the situation, but only as a modifying factor. The stimulus is treated as the datum or starting point, while the resulting behavior or conduct is treated as the end point for the psychological inquiry. The medical writers on psychology state or imply a very different analysis of conduct. Here, the starting point of conduct is the individual person himself. He wants certain things, he has cravings, desires, wishes, aspirations, ambitions, impulses. He expresses these impulses in terms of the environment. The stimulus is treated by the new psychology as only a means to an end, a means utilized by the person in getting the satisfactions that he intrinsically wants. This is a very basic contrast. In the older schools of psychology we have the characteristic formula: the stimulus—the person—the behavior. Behavior is thought of mainly as replies to stimuli. In the newer schools of psychology we have a different characteristic formula: the person—the stimulus—the behavior. The stimulus is treated merely as the environmental fact that is used to express purposes.

In the psychology which is current in the colleges and normal schools, we teach a stimulus-response formula about which everything else psychological revolves. The contributions of the newer schools of psychology are certain to

modify the rigidity of this formula. By the stimulus-response formula is meant the constant resolution of every psychological problem into three conventional parts; the *stimulus*, which is treated as a first cause, the *mind or central nervous system*, and the *behavior*, which is treated as a reply to the stimulus. After some practice this formula becomes so thoroughly ingrained that every psychological question is habitually broken up into a search for the provocative stimulus, a description of the resulting mental states, and a description of the responsive behavior. Often only the first and last of these facts are looked for.

Psychology as a Science

Every scientific problem is a search for the functional relation between two or more variables. This can be seen very clearly in the exact sciences, but it becomes more obscure as we enter the biological sciences, and it is frequently lost sight of in the social sciences. In physics we have, as typical problems, the search for the relation between the length of time that a body has been falling and its speed, the pressure of a gas and its temperature or volume, the curvature of a lens and its focal distance, the resistance of a wire as determined by its cross section and length, the pressure on a turbine as determined by the head in the penstock, the sag of a beam as determined by the load and the cross-section of the beam. Physicists and engineers come to look upon the search for relations between variables as the typical task of science. The attitude of looking for these relations becomes second nature to them. They reach habitually for a piece of cross-section paper in order to make a graph of their observations, and in order to visualize the nature of the relation that they are seeking.

In the biological sciences we have the same logic in the biometric methods. In the social sciences we are dealing, usually, with variables that are not quantitative, but there is no good reason why thinking in the social sciences should not follow the same logic even though the variables are often non-quantitative.

If every scientific problem is an attempt to state the relation between two or more variables, it should be profitable to note what the variables are that constitute scientific psychology. If we look over the field of experimental psychology, as it is represented by standard textbooks in the field, and by the work in college psychological laboratories, we find that most of the relations into which the experimenters inquire classify themselves in the following types: (1) relation between anatomy of the sense organs and conscious sense experience, (2) relation between stimuli and sense experience, (3) relation between stimuli and muscular adjustment.

Totally different is the fundamental nature of the relations that the medical writers in psychology are dealing with. They are constantly searching for the relation between the fundamental cravings and wants of people and the ways in which these wants are expressed and satisfied. A patient talks and behaves as though he were an emperor, a millionaire, a person with power and fame. Let us contrast the two different scientific approaches of the old and the new psychology to this problem. What are the variables involved in the problem? The psychologist of the established academic schools would ask about the stimuli, the environment, and he would state or imply in his solution that the patient is merely responding to stimuli. There might be some difficulty in specifying just what the stimuli are to which the patient is responding by talking like an emperor. The academic psychologist would list on one side of his scientific ledger the stimuli and environment to which the patient has been exposed, and on the other side of the ledger would be recorded the behavior of the patient. The conduct would be described as a function of the environment, modified, to be sure, by the characteristics of the patient himself.

The psychiatrist would look for a different set of variables. He would list on one side of his ledger the wants and cravings of normal people, assuming that these wants are also a part of the self of the patient, and on the other side of the ledger he would list the patient's conduct. The scientific problem

would be to state how it comes about that the patient expresses in his particular way wants that are universal. The psychiatrist would treat the environment as merely the means by which the patient seeks to express wants and cravings that are universal. This procedure is much more powerful and illuminating. It shows us more about human nature, but it is not subject to the exact quantitative technique of the older sciences because the wants and cravings of normal people have not yet been classified and isolated in a *measurable* way.

Since incentives and desires are not readily measured we rest content with describing the relations that we *can* measure. It would not be subject to criticism if it were not for the fact that we have come to forget the individual person altogether. Experiments of this type have come to be the rule and we have taken for granted that psychology is primarily concerned with the incidental relation between the response and the response-modifying stimulus. We have gone so far as to assert that psychology studies the stimulus-response relation, and we have forgotten the person himself who may or may not want to do the responding.

Summary

The contribution of psychoanalysis to theoretical psychology may be summarized under two heads that we may interpret as implied criticisms of us.

1. We have devoted ourselves almost exclusively to the phenomena of *momentary mental states and momentary behavior* whereas the psychoanalysts have given their labors to the phenomena of *basic and permanent tendencies in human nature*. Our analyses of instincts are superficial and merely descriptive. The study of our psychology does not gain for us, and for our students, any greatly increased understanding of human nature. The psychoanalysts at least work consciously for such insight as their objective.

2. We have devoted ourselves to the scientific study of the relation between stimulus and response, environment and conduct. This is an incidental relation. It is not the

basic one for the understanding of mind and behavior. The psychoanalysts devote themselves to the study of the more fundamental relation between the demands of the organism which constitute the essence of its life on the one hand, and the behavior by which the organism satisfies these demands. The stimulus or environment is then only the momentary means by which the organism lives at any particular moment.

As an example of the effect of psychoanalysis on theoretical psychology consider the social and emotional phenomena of compensation which have been discussed at length by Adler and other psychoanalytic writers. I turned to the indexes of twenty standard textbooks of psychology and I found the term 'compensation' in two of them. In one of these textbooks the term refers to olfactory and gustatory compensation, and in the other it refers to the compensatory reflexes controlled by the semicircular canals. Not one of these textbooks mentioned compensation as of social or emotional significance.

The psychoanalyst uses the term 'compensation' not only for such momentary phenomena as the reflexes, but he applies the same concept to understand compensatory effects in the formation of personality. These social interpretations of compensation are beginning to receive the attention of scientific psychologists, in respectable psychological journals. While we disapprove of the unscientific methods of the psychoanalysts, we should not deny that many of our most useful ideas in social and theoretical psychology have their origin in psychoanalysis. Other psychoanalytic concepts are gaining admission to scientific psychology, and we should be fair enough to acknowledge the source from which we are taking them.

The psychologist is frequently disgusted with the psychoanalysts and with the psychiatrists because these men seldom have any regard for scientific method. It is true that they often let their imaginations run wild. They seldom express themselves clearly. They rarely define their terms. They almost pride themselves on loose thinking. They know nothing about controlled experimentation. No wonder that

scientists are disgusted with them. The psychoanalyst or the psychiatrist to whom these criticisms do not apply is a rare person.

But let us not be so petty that we refuse to acknowledge merit even though it be closely associated with gross defects. The psychoanalysts have made a contribution to theoretical psychology in calling our attention to gross deficiencies in what we call the subject-matter of psychology. The content of psychoanalysis, psychiatry, and of the so-called new psychology is much more important than the content with which we have busied ourselves as scientific psychologists. The underlying relation between the life demands of the organism and the behavior by which these life demands are satisfied is the subject of psychoanalytic study, and that relation is more important as a determinant of mental life, personality, and conduct, than the stimulus-response relation to which we as scientific psychologists have given most of our effort.

Let us turn to the effects which the psychoanalysts and the psychiatrists have used on an empirical basis in their medical practice and apply to these phenomena the methods of controlled scientific experimentation. It is certain that both medical practice and psychology as a science will profit from such a venture.

CONTRIBUTIONS OF FREUDISM TO PSYCHOLOGY

II. Freudian Psychology and Scientific Inspiration

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When the Freudian movement is regarded from a distance sufficient to obscure its secondary aspects it appears, on the whole and in spite of crude and extravagant features, to fall in with trends that are regarded in orthodox psychology as progressive. I shall spend only the first and the lesser part of my time in stating very briefly four tendencies of Freudism that appear to me of value to psychology.

1. We may first of all welcome this so-called new psychology because of the impetus it has added to the movement away from an atomistic and intellectualistic psychology and towards a thoroughly integrated and dynamic conception of man—a movement already well under way. Freudism is admirably free from the fallacies of associationism and from the exclusively static and structural point of view. In the more general sense of the terms, the Freudians are functionalists and behaviorists, *i.e.* they are interested altogether in action and its motivation.

2. Their psychology is radically deterministic—NOT perhaps more so in theory than the psychology they call 'old,' but certainly more so in practice. It holds that nothing in the psychical life, or almost nothing, is a matter of chance; slips of the tongue and of the pen, forgetting, misplacing, dreams, as well as the apparently meaningless manifestations of psychical disease, have a meaning, a degree of rationality; they are held to be determined by psychical antecedents which are usually wishes or desires balked or repressed.

3. Regarding the persistency, in some form or other, of psychical impressions, and their potency upon later behavior, the Freudians have much to teach that is valuable. For the average classical psychologist, most of what happens to

any individual is soon forgotten and, when beyond recall, is without influence of any sort upon his behavior. The Freudians are, in my opinion, rendering an important service in forcefully directing attention to the almost limitless possibilities of recall and especially to the specific influence upon behavior of unrecalled or unrecallable experience.

I do not endorse their theory of a conscious subconscious. I merely express agreement with them in the belief that the past, even though it be neither recalled nor recallable, continues to exert a definite, specific influence upon present behavior.

Whether the persistency of forgotten experience be considered as due entirely to a continuation of its physiological effects or not, it must be recognized, I think, that behavior is at any moment the result not merely of the motives actually present to consciousness but also of the total past. I use the term 'total past' not merely in the vague, generally accepted sense, but in something like the specific and detailed manner in which the Freudians understand it.

4. Should we not be grateful to Freud, Jung, and their disciples—however ill considered and extravagant their theories may be—for having thrown wide open the doors leading to great and central problems of personality and behavior? This adventurous, unconventional band of physicians, following upon the pioneer, yet more scientific, work of Charcot and Janet, have renewed the interest in psychology not only of the public but also of many a professional psychologist. After all, do not most of us admit that, before the advent of the mental tests and of Freudism, Psychology was threatened with dry rot? Our task now is not to let our science be either absorbed or corrupted by these aggressive and clamorous infants.

One of the main problems of the Freudian system concerns the nature of the subconscious forces upon which much in normal and abnormal behavior depends. I propose to spend the rest of my time in showing what may be the nature of the subconscious activity in an instance of what is popularly called scientific inspiration or revelation.

In an article on invention in mathematics the great French mathematician, Henri Poincaré makes the surprising remark that the most striking feature of mathematical invention is 'apparent, sudden illumination.' Of one of the greatest of his discoveries, the *fonctions Fuchsiennes*, he wrote:

"I had been endeavoring for two weeks to demonstrate that there could exist no function analogous to those I have since called *fonctions Fuchsiennes*. Each day I spent an hour or two at my working table . . . but I came to no solution. One evening, contrary to my habit, I drank some black coffee. I could not sleep; ideas crowded in my mind; I felt them knocking against one another, until two of them hung together, as it were, and formed a stable combination. In the morning, I had established the existence of a class of *fonctions Fuchsiennes*. There remained merely to set down the results and that was done in a few hours."

This accomplished, he set about exploring systematically the new domain brought to view by the discovery. In the course of that exploration a problem arose which again stubbornly resisted solution: "My efforts served only to give me a fuller knowledge of the difficulty—that was already something gained. So far, my work was entirely conscious. Thereupon, I left for Mont Valérien, where I was to be during my military service; my preoccupations became therefore very different. One day, as I was crossing the Boulevard, the solution of the difficulty appeared suddenly." He found himself in possession of all the elements for the solution. Nothing remained for him to do but to bring them together and to organize them. This he did, as he says, 'at one sitting and without any trouble whatsoever.' [1]

Of another mathematical discovery, made while out walking, he wrote that it came to him 'with the accustomed traits: brevity, suddenness, and immediate certitude.' [2]

An equally remarkable instance of scientific inspiration, is the discovery of the Quaternions by Sir Wm. Rowan Hamilton. It came to him, while walking with Lady Hamilton. They came up to Brougham Bridge near Dublin. He

[1] Henri Poincaré, 'Science et Méthode,' Paris, 1920, pp., 50–51, 53.
[2] *Ibid.*, p. 52.

pulled out a pocket book and wrote down the 'fundamental equations between *i*, *j*, and *k*, exactly such as I have used them ever since.' He felt 'the galvanic circuit of thought close,' and the relief of an intellectual want.

Whatever its explanation, the fact itself has to be accepted. In artistic as in scientific discovery, *i.e.* both in the field of imagination and of rational construction, there come, after periods of mental striving or vague brooding, fructifying moments, effortless and unexpected, which give the impression of inspiration.

But it is not only the solution of great problems that appear unexpectedly; *all* kinds of ideas, and ideas of all degrees of puerility and importance, appear in our minds under the conditions which we have found to be those of revelation. "The Ballad of the Schooner 'Hesperus'" which flowed from Longfellow's pen by stanzas, without effort, does not embody any great thought. Mozart seems to have claimed that all, and not only his remarkable, musical compositions came to him unexpectedly.

But why look so far for illustrations? Almost every moment of conscious life provides everyone of us with similar facts. The common instances of 'bright' ideas, of happy thoughts, which offer themselves when we have ceased to seek them, are disconnected from the train of thought of the moment and seem not the reward of effort but gifts from unknown sources.

What we affirm is that the mental processes of inspirational invention and of ordinary thinking are essentially similar. It is a complete misrepresentation of thought to picture it as gaining its ends by a straightforward, uninterrupted flowing movement. Conscious processes are, on the contrary, full of stops, of breaks, and of sudden forward leaps. They are like a fire which seems to go out when blown upon, and which spasmodically flares up again when forgotten. It is often when vain effort has compelled us to give up that an illumination surprises us. The revivalist admonishes the repentant soul to let go, to surrender to the arms of Jesus; then salvation may come. The mystic, likewise, seeks the

Divine in passivity. Old Egyptian wisdom had already reduced the truth involved in these practices to an aphorism: "The archer hitteth the target partly by pulling, partly by letting go; the boatman reacheth the landing partly by pulling, partly by letting go." [1]

Thought proceeds very much like the formation of the chain we have all seen coming into existence on the screen of a moving-picture theatre. Each link appears separately and jumps into place suddenly. There is no more continuity in thinking than in the formation of that continuous chain. For a time the strain of the purpose seems to act as a center which attracts to itself, as it were, the various elements of the problem. These elements appear most irregularly. There are moments when no progress is made; attention relaxes and turns in desultory fashion to other things. Suddenly a new link pops into consciousness and adds itself to the chain. Then the directing purpose may again be felt and the double process of effort and relaxation repeats itself. The interruptions may be so brief as to be unnoticed, and then, remaining under the impression of the effort, we assume that the idea appeared during the attentive phase.[2]

When Poincaré remarks that during his military service he was occupied with matters other than mathematical problems, we are not to understand him as affirming that the problem of the *fonctions Fuchsiennes*, with which he had been long and profoundly engaged, never crossed his mind. Sir Wm. Rowan Hamilton states that during the weeks preceding the revelation, active interest in the problem of Quaternions had revived, and that as he was approaching the bridge, although he was talking 'now and then' with Lady Hamilton, yet in his mind 'an undercurrent of thought was going on' which suddenly flared up into the memorable equations. When these undercurrents of thought—brief and weak as they often are—have no particular importance, they are disregarded and forgotten.

[1] From the instructions of Ptah Hotep to his son. Quoted by Hocking in 'The Meaning of God in Human Experience,' p. 419.

[2] In differing as we do here with Wm. James' famous chapter on the 'Stream of Consciousness,' we are not to be understood as lapsing into the physical atomism against which he was setting himself.

The moments of interrupted attention are filled with nothing at all, or with thoughts and feelings belonging to another topic; we may simply look up, finger our eyeglasses, consult the clock, light a cigarette, and presto, the idea we had ceased to seek is present. Again, the arrested activity—the passive phase of the process—may be protracted and the task given up for the present. A week or a month later a constructive thought may suddenly and unexpectedly appear and may lead to a speedy solution of the problem.

The problem of inspiration, illumination, revelation—call it what you will—does not, then, refer only to very remarkable and rare occurrences. The traits by which revelation is commonly separated from ordinary, natural, human products are in various degrees characteristic of all thought and action. Unexpectedness, absence of effort, passivity, and also clearness and certainty, may belong alike to the great and the small, the true and the false, the religious and the secular.

We have so far merely described and classified scientific revelation. We may now explain why a long break in the attention-effort is, at times, the precursor of a surprising forward movement.

It happens occasionally that after a sufficiently long rest from an activity involving movements (typewriting, for instance), on resuming the practice it seems that an improvement has taken place. As a matter of fact, improvement after a long rest has been observed under experimental conditions. W. F. Book reports the following observations:

A subject practiced typewriting until he made, during the last ten-minute practices, the average score of 1508 words per period of ten minutes. After an interval of six months, he was tested ten times, ten minutes each time, under exactly the same conditions as those prevailing during the practice. The average score was 1433 and the number of errors was greater than during the last practice series. He refrained again from using the typewriter, this time, for a whole year. Thus, one year and a half had elapsed between the cessation of the practice and a second memory test. During this second test, the average score for ten ten-minute periods was

1611 words and the percentage of errors was less than during the first memory test. "There seems," says Book, "to have been an actual increase in skill during the rest interval of one year and a half. How is this to be explained? The increase in score shown by our second series was due, so far as we could make out, rather to the disappearance, with the lapse of time, of numerous interfering associations, bad habits of attention incidentally acquired in the course of learning, interfering habits and tendencies, which, as they faded, left the more firmly established typewriting associations free to act. Such hindering associations were developed in all stages of practice and at the 'critical stages' in great masses, forming a serious impediment to progress. After the rest of a year and a half these conflicting associations and hindering tendencies had noticeably disappeared." The six months which elapsed between the last practice and the first memory test were not sufficient, in the opinion of Book, to permit the disappearance of the hindering associations, hence the lowered score. "A year later, during the second memory test, the absence of difficulties and the greater ease had become so prominent as to attract the attention of the learner. The errors have slightly decreased and the score is better than ever before. We, therefore, conclude that it was the disappearance of the interfering associations and tendencies naturally developed in the course of the learning which caused the increase in the score." [1]

How one may pass from the above instance of improvement in typewriting to the explanation of scientific inspiration, it is easy to see. Thinking, as well as typewriting, involves a neuromuscular mechanism. Our thoughts assume commonly a verbal form, even when they are not expressed in audible speech or in writing. The merely 'mental' formulation of thought does not take place without incipient innervation of the speech and of other mechanisms.

We may, therefore, say that thinking, like typewriting, involves false moves. As we repeat the unprofitable thinking,

[1] Book, W. F., 'The Psychology of Skill: with Special Reference to its Acquisition in Typewriting,' *Univ. Montana Public. in Psychol., Bull.* No. 53, *Psychol. Series* No. 1. Summarized in Thorndike's 'Educational Psychology,' Vol. II., pp. 311–317.

while exploring blind alleys, the production of the right thought becomes increasingly difficult. We all know that in certain circumstances it seems as if the mind had become limited to wrong directions; it goes round and round in the same vicious circles. If at such times we let go, thus producing a condition that will make possible a weakening or a disappearance of the unprofitable thought-movements, and subsequently return to the problem, we stand a better chance of striking a new path, and the new path may be the right one.

Thus we may understand why the scientist, the philosopher, and other persons are at times surprised by the appearance of fruitful ideas which strenuous efforts had failed to produce. Has the effort been useless? Certainly not. Or let us rather answer, *some* of the effort and perhaps all of it was necessary. Had not the problem been examined, the solution would not, so far as the facts can be read, have been secured after relaxation.

Possibly it is not useless to repeat that remarkable inspirations of the sort I have reported are very rare, and that usually a return of the problem does not lead to a solution; wrong movements are again made.

The description of scientific revelation has disclosed to us its fundamental identity with the mental processes of ordinary productive thinking. Whatever differences exist are differences of degree and, on the whole, unimportant. We have also obtained what seems to us a satisfactory explanation of the way—or at least of one of the ways—in which relaxation, passivity, and rest favor the appearance of new thoughts, and thus the unexpected solution of problems. No subconscious mental activity of any sort is involved in this explanation, but only the fading out of physiological impressions, whatever their nature may be.

There remains the more difficult and fundamental problem of the formation of new thoughts. Whence do they come, how are they created, and why do they appear when they do? These are essential problems of mental production which it is not our task to discuss. We have limited ourselves to showing that in at least one class of facts, for the explanation of which it is usual to posit an unconscious *mental* activity, another adequate explanation is at hand.

CONTRIBUTIONS OF FREUDISM TO PSYCHOLOGY

III. Physiological Analysis of the Libido

BY K. S. LASHLEY

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In the previous discussions the contributions of psychoanalysis to dynamic psychology have been emphasized. It is true that the psychoanalytic schools have presented a definite theory of the nature of motivation in behavior and have sought to apply it to almost every field of human activity but in the construction of their theory the psychoanalysts have overlooked or willfully ignored a large amount of data which other branches of psychology have contributed to the solution of this same problem. In particular they have disregarded the data of psychobiology; the theory has been developed in terms of psychic forces with scarcely a reference to the biological basis of the phenomena. This might be permissible if mental mechanisms could be absolutely divorced from physiological problems, but such a claim has not been made even by the most extreme of the adherents of the new psychology. Freud himself admitsthat the psychic forces are physiological in origin. Speaking of the mechanism of repression and displacement he says, "The mechanism of these processes is entirely unknown to me. Anyone who wishes to follow up these ideas must try to find the physical analogies and prepare the way for a demonstration of the process of motion in the neuron."[1] Again, "It is not the psychic formation that appears to us as the moving factor, but the innervation of the same." Thus Freud admits a physiological basis for the mental mechanisms and an examination of these mechanisms shows that they imply a whole system of physiological psychology. In accepting psychoanalytic explanations we are in some danger of incorporating the Freudian

[1] Freud, S. 'Interpretation of Dreams,' New York, 1913, p. 475.

physiology into our science, and this uncritically because it has not been stated explicitly.

The dynamic conceptions involved in the doctrines of libido and censor are fundamental to psychoanalytic explanation of behavior. The libido is a real force for the psychoanalysts, not merely a name for the unexplained facts of motivation. Some of the principal points of this dynamic theory are these: First, behavior is motivated by forces or energies which are derived from some innate mechanism in the organism and are exhibited in the interplay of the ego complex and libido. Second, the energy of the libido behaves as a unit; that is, it may vary in quantity but not in kind; the entire energy may be available for motivation of any one of a number of activities. It is conceived as free energy. This is fundamental to the conceptions of sublimation and conversion. Third, the energy of the libido is derived from a single source, the sex instinct of Freud, vital force of Jung, masculine protest of Adler. A few psychoanalysts, as Jeliffe and Kempf, have assumed a variety of libidos or a multiple origin for the forces, but their laws of action are practically identical for all the forces, and do not escape the objections which apply to the other systems. Fourth, the energy of the libido may be rendered latent by the opposition of other energy (that of the censor or ego complex) as exhibited in conflict and repression. Fifth, when held in check, the energy of the libido accumulates and exerts increasing pressure against the restraining agent until it finally escapes in the dream, hysterical fugue or the like. Sixth, the energy may be drained off in a variety of ways, any one of which will relieve the general pressure. It may be redirected into socially permissible activity in sublimation, may force its way out in the form of hysterical symptoms, or may be converted into fear.

These dynamic principles have been deduced from phenomena of complex behavior which are perhaps capable of quite different interpretation. They have not been related to any of the simpler processes in the organism and have made no use of studies of motivation based upon other methods than

the psychoanalytic. Instead of employing the available and pertinent facts of neurophysiology the writers are content with a crude mechanics which seems to have been derived from analogy with the simplest physical forces. The mental mechanisms of the psychoanalysts resemble more closely the behavior of liquids under pressure than they do any physiological processes and, indeed, the similarity is so exact that we might justly call the Freudian dynamics a system of psychohydraulics.

Do these assumptions of the psychoanalysts agree with what we know of motivation in behavior through studies employing a different method? Is there evidence that the activities of the organism are motivated by a common energy? Is there evidence that the energy is derived from a single source? That this source is the sex instinct? Is there justification for believing that any such energy or energies exist at all?

In view of the psychoanalyst's emphasis on sex, the most conclusive evidence on these points is to be sought in the simpler sexual activities and there have recently appeared some important studies of sexual motivation in animals which bear directly upon the problem. Sex motivation in man is certainly not identical with motivation in lower mammals, yet the differences seem to be of degree rather than of kind. There are but few physiological data for man, so that the only certain data, come from the animal work.

The studies of Steinach, Moore, Stone, and others [1] give a rather complete picture of the mechanism of the sexual reactions of the male rat and a review of this work, in relation to psychoanalytic theory will, I think, be profitable. I shall first state briefly the facts which the work has revealed.

The mature male is thrown into sexual excitement by a complex but very well defined pattern of stimuli. The pattern may be wholly kinesthetic and is that presented by contact with a small object moving with a series of quick

[1] For a review and bibliography of much of this work cf. Stone, C. P., 'Experimental Studies of Two Important Factors Underlying Masculine Sexual Behavior: the Nervous System and the Internal Secretion of the Testis.' J. *Exper. Psychol.*, 1923, 6, 85–106.

jerks. Stone has shown that the reaction is independent of vision, audition, smell, taste, or cutaneous excitability.[1] The response to this adequate stimulus is increased activity, pursuit of the stimulating object, seizing with the fore legs, and copulatory movements. The behavior of the adult is thus reducible to stimulus response relationships, complex but nevertheless clearly reflex in principle.

The appearance of the reaction is conditioned by other factors than the adequate stimulus. The history of the development of sexual behavior in the individual shows that up to puberty the animal is not excitable by this adequate stimulus. The preadolescent males are in no way excited by females in heat. Neither the copulatory pattern nor any acts which might be interpreted as a foreshadowing of it appear in them. At the time of maturation of the germ cells and increase in interstitial tissue, the animal is suddenly sensitized to the adequate stimulus. He now reacts to the female in heat and shows the copulatory pattern, fully integrated at its first appearance. Further work has given us an insight into the character of this sensitization. Castration before puberty or after, if the animal has not had sexual experience, prevents the appearance of the sexual reactions. Castration after puberty and sexual experience leads to their loss after a brief interval. Grafting of testicular tissue, or even subcutaneous injection of mascerated testis reëstablishes sexual activity. The internal secretory action of the testis in this is clearly established.

There are three possible ways in which the autacoid might act. First, it might increase the general excitability of the animal, either by changing metabolism, by increasing the tonus controlled by the vegetative nervous system, or by a direct general action upon the central nervous system. This might be interpreted as the supplying of free libidinous energy, but the evidence is against such an hypothesis. We have tested the general activity of castrates as measured in revolving cages, in problem solving, and learning and find them

[1] Stone, C. P., 'Further Study of Sensory Functions in the Activation of Sexual Behavior in the Young Male Albino Rat,' *J. Comp. Psychol.*, 1923, 3, 469–473.

not different from normals. The autacoid does not act as a general motivating agent.

A second possibility is that the autacoid might act upon the autonomic nervous system of the pelvic region to increase the tonus of the pelvic organs, and so, through their afferent nerve supply, facilitate central nervous mechanisms and lead to sexual activity. This is the mechanism which Kempf's theory [1] of segmental strivings demands. It is definitely ruled out by the following facts: Stone has shown that the castration may involve the removal of the testis, epididimus, vas deferens, seminal vesicles and all of the accessory sex glands, and at least a partial denervation of the penis and scrotum—that is, the elimination of practically every organ which could contribute to the pelvic segmental striving—yet the sensitization by implanted testis occurs. Moore [2] has shown that the grafting of ovaries into the castrated male induces typical feminine behavior, in spite of the fact that all of the organs which could give rise to segmental strivings are male in character. Late work on gynandromorphs in insects[3] indicates that their sexual behavior is determined by the sex of the head, even when the head has the characters of one sex and all the rest of the body has those of the other. On the basis of such evidence we must reject Kempf's theory of the somatic character of libidinous strivings.

The third possibility is that of the direct action of the autacoid upon the central nervous system. It cannot there generate energy, for the doctrines of nervous energy have been most thoroughly exploded by the last ten years' work on the nature of the nerve impulse. We are forced to the conclusion that it acts in some way to lower the resistance of definite reflex pathways so as to integrate or make excitable the reflex mechanisms of the sexual reactions, and the effect seems to be specific upon these, leaving other reaction systems

[1] Kempf, E. J., 'The Autonomic Functions and the Personality,' *Nervous and Mental Disease Monog.* No. 28.

[2] Moore, C. R., 'On the Physiological Properties of the Gonads as Controllers of Somatic and Psychical Characteristics. I. The Rat, *J. Exper. Zool.*, 1919, **28**, 137–160.

[3] Whiting, P. W. 'Conflict of Instincts in Gynandromorphs of *Habrobracon*,' *Anat. Rec.*, 1923, **26**, 395.

unaffected. This hypothesis seems more in accord with the known facts than does either of the others and it is not contradicted by what we know of the action of hormones in other phases of the development of the nervous system.

In addition to the definite copulatory pattern of reaction the animal shows other changes in behavior of a more general character. The first presentations of the adequate stimulus seem merely to increase excitability by the stimuli from other animals in the cage. There ensues a period of increasing activity culminating in the copulatory reaction. At the same time there is a decrease in the sensitivity to food, odors and the like. With increasing excitement, the adequate stimulus is simplified and at the height of the excitement the male may attempt to mate with other males, or in fact, with practically any small moving object.

The mechanism of this excitement is obscure, but we can not ascribe it to the presence of free libidinous energy, for all the arguments which apply against that conception in connection with the autacoid also apply here. The situation is closely parallel to what we see during the formation of the conditioned reflex where in the first stages the reflex is undifferentiated and the conditioned reaction may be excited by almost any type of stimulation. In the conditioned reflex we are almost certainly dealing with an increased excitability of the final common path, resulting from repeated stimulation. It is probable that a similar mechanism is involved in the development of sexual excitement. I shall revert to this point later.

The sexual reactions are conditioned by still other factors than these. Severe undernutrition abolishes them, as does probably thyroidectomy, and perhaps disorders of other endocrine organs. The evidence is not clear here but it seems most probable that these agents act by a general reduction of reflex excitability through disturbances of metabolism and have no direct bearing on the problem of sexual motivation.[1] Sensory stimulation, as hunger contractions, proto-

[1] There is evidence that starvation and some endocrine disorders are followed by regressive changes in the gonads, so that the action of these agents may be only indirect, through the modification of the sex hormones.

pathic stimuli, even a strange environment, will inhibit the sexual reactions. A severe mauling by a large female during his first sexual advances may postpone indefinitely the appearance of sex activity in a small male. Practice in sexual activity, on the other hand, tends to maintain potency for a short time in the absence of the autacoid.

Different phases of sexual behavior seem to be independently conditioned. Moore's work shows that masculine or feminine behavior depends upon a specific autacoid in each case. There is some evidence that the care of the young by the female is conditioned by pregnancy and parturition and is independent of the conditions determining other phases of her sex activity.

All this evidence points to one conclusion concerning the nature of the sexual reactions. There is no sex instinct or sex drive in the meaning with which the words are ordinarily used. Sexual behavior consists of a number of acts, each a definite response to a definite pattern of stimulation. These reflexes are independently conditioned by specific hormones, by nutritional factors, and by habitual modifications. There is no evidence here for the existence of free energy. There is no unity among the sexual reactions such as is essential to prove their common motivation. There is no source of energy which could fulfill the requirements of the libido.

The psychoanalytic theories do not fit the facts in this case. Before we ask whether they account more adequately for the facts in man we must consider other possible types of motivation. The clearest case for the conception of motivation or drive comes from sensory facilitation. Protopathic stimuli increase general activity in certain ways. Under punishment the animal gives avoiding reactions which involve increased responsiveness to certain elements of his environment, habitual reactions to various avenues of escape or random struggles. The explanation of this increased sensitiveness is provided by the facts of spinal irradiation, which show that spread of neural activity on protopathic stimulation results in reflex facilitation.

In this same category belong the excitations from the

hollow viscera. The work of Carlson,[1] and especially that of Richter[2] and of Wada[3] shows that activity is facilitated by hunger contractions. Similar effects of rectal and bladder contractions may be observed. But the evidence indicates that the facilitation is limited to the duration of the contractions and is limited also in the kinds of activity facilitated.

Habits may be formed under the influence of such facilitation but after they are formed, they function without it. Thus Dodson's work[4] indicates that hunger contractions are effective in maze learning in proportion to their intensity, yet after the animal has learned the problem he will perform the habit, though fed to repletion. Further, this type of mechanism seems limited to the digestive and excretory functions and is not a universal principle of motivation, as Kempf has assumed. We have seen that nothing of the sort is involved in sex activity, and it seems equally certain that social reactions of other sorts, as the gregarious responses of the chick are not so activated.

Is there evidence for any other source of energy in the organism? We have heard much of glands in this connection. I have outlined the data on the sex glands. Cannon's work on the suprarenals shows that they do not provide a mechanism for specific drives. In our own work we have found that their complete removal from the rat leaves the animal's behavior relatively unaffected. Most of the psychological speculation concerning the dynamic function of other glands is as yet unsupported by any experimental evidence.

Facilitation by postural reflexes undoubtedly plays a large part in determining behavior, but I believe that the weight of evidence indicates that the postural reflexes are of relatively short duration. They may account for some of the dynamic effects of emotion and for such conceptions as

[1] Carlson, A. J., 'The Control of Hunger in Health and Disease,' Chicago, 1916.

[2] Richter, C. P., 'A Behavioristic Study of the Behavior of the Rat,' *Comp. Psychol. Monog.*, 1922, 1 (No. 2), 1–55.

[3] Wada, Tomi, 'An Experimental Study of Hunger in its Relation to Activity,' *Arch. Psychol.*, 1922, **57**.

[4] Dodson, J. D., 'Relative Values of Reward and Punishment in Habit Formation,' *Psychobiology*, 1917, 1, 231–276.

'set,' but they cannot be appealed to as the source of long continued drives.[1]

The older doctrines of nervous energy provided a beautiful mechanism for the psychoanalytic theories, but the work of Verworn, Lucas, Adrian, Forbes, Piper, and other students of nerve conduction definitely invalidates these doctrines, and leaves us only anatomical organization, relative thresholds of conduction, facilitation, and inhibition as established neural principles for explanation of behavior. No free energy, no physical basis for psychic forces has been discovered.[2]

This review of physiological data has been necessarily, only a brief sketch of sources of evidence, but a more complete survey of the physiology of behavior only confirms, I believe, the evidence against the energetic conception of behavior. There is no known mechanism in the organism which can meet the demands of the Freudian psycho-hydraulics.

Do the facts presented by psychoanalysis demand that we subscribe to its dynamic theories in spite of the contradiction of physiological evidence? Psychoanalysis presents an elaborate and fairly self-consistent explanatory system. I should hesitate to attack it *in toto*, were it not for the fact that the recent revolt against the concept of instinct shows that psychologists are, on the whole, opposed to the appeal to mysterious forces as explanations in behavior; and the psychoanalytic mechanisms involve just such conceptions as have led to severe criticisms of the social applications of instinct. Psychoanalysis provides no direct evidence for the existence of libidinous or other sorts of energy. The justification for the hypothesis is that it appears to explain a very large mass of data.[3] But if other explanatory principles can be found which equally cover the facts of behavior and are in better accord with physiological principles, the psychoanalytic

[1] No one has observed such enduring postural changes except perhaps in the case of hysterical contractures, and to cite the latter as evidence for continued facilitation by postural reflexes is to explain the disease in terms of its symptoms.

[2] Adrian, E. D., 'The Conception of Nervous and Mental Energy,' *Brit. J. Psychol.*, 1923, 14, 121–125.

[3] The quality of the explanation is perhaps irrelevant. Personally, I am as much mystified by such statements as 'the gaps in the manifest dream are the fault of the dream censor,' as I am by the gaps themselves.

interpretations must give way to them. It is simply a question of accepting the hypothesis which synthesizes the greater number of facts.

Such principles have been suggested by Woodworth,[1] Watson,[2] and Southard,[3] among recent writers. They make use of conditioned emotional reactions, failure to develop social habits, or the development of antisocial ones, conflict of habit systems, and emphasize the importance of constant social irritations, as the psychoanalysts have not done. The conceptions have not been elaborated as have those of Freud, but where they have been applied they provide at least as satisfactory an explanation, without violating our conception of physiological organization.

Rather than attempt to elaborate these principles, a task for which I am ill qualified, I should like to refer to a line of neurological investigation which has received little attention, but which seems to offer a neurological basis for Woodworth's view that a habit may become a drive and so to offer a more satisfactory alternative to the Freudian dynamic theory. Martin[4] has recently pointed out that in vasomotor reflexes, the nature of the reaction depends upon the total number of nerve impulses reaching a center, rather than upon their pattern. Strong stimulation of a few fibers has the same effect as weak stimulation of many. This illustrates a law of mass action which seems also to apply in cerebral function.

In the cerebrum, partial destruction of a functional area leads, not to the loss of a few reactions, but to difficulty in the performance of many, and the difficulty seems in some degree proportional to the amount of destruction and involves primarily closely related habits. We can interpret this, I believe, only as showing that the elements of organized systems of habits involve essentially the same cerebral mechanisms or widely overlapping mechanisms which exert a

[1] Woodworth, R. S., 'Dynamic Psychology.'

[2] Watson, J. B., 'Behavior and the Concept of Mental Disease,' *J. Phil., Psychol.*, 1916, 13, 589–598.

[3] Southard, E. E., 'The Kingdom of Evils.'

[4] Martin, E. G., 'The application of the 'all or nothing' principle of nervous conduction to the interpretation of vasomotor reflexes,' *Amer. J. Physiol.*, 1922, 59, 400–412.

mutual facilitation. That is, stimulation to any response seems to involve both an activation of specific conditioned reflex paths, and also a partial activation of many paths which function in associated habits. The whole system is thrown into partial activity and facilitates the specific response. The ease with which any reaction can be elicited depends upon the amount of functional tissue available and contributing to the facilitation, and hence upon the number and complexity of the associated activities. Work on this problem is only begun, but the hypothesis receives support from many sources and seems to offer an alternative dynamic principle which avoids the psychoanalytic energetics and provides a mechanism for those cases which seem to demand the assumption of some sort of 'drive.'

To sum up, the psychoanalysts have developed a crude mechanistic system of explanation based upon analogy with simple physical forces and with complete disregard of physiological facts which bear directly upon their problems. Their explanations, in so far as they are based upon the conception of physical or vital energy, are flatly contradicted by physiological evidence. Psychoanalysis has done important work in emphasizing and systematizing problems and has given to psychology such valuable categories for classification of types of behavior as conflict, rationalization, and the like, but the dynamic principles which have been advanced to explain the action of the Freudian mental mechanisms are scientifically unsound. The problem of motivation is far more complex than the Freudians would have us believe and its solution is to be sought in the investigation of many related fields: the analysis of specific instinctive responses, the neural basis of emotions, the mutual influence of habits, the total integration of all such systems of reaction. The hasty postulation of such crude vital forces as the libido can only delay experimental investigation and postpone a real insight into the true nature of human motives.

CONTRIBUTIONS OF FREUDISM TO PSYCHOLOGY

IV. The Neurological Concept of Behavior

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Under this designation of a common point of view, I propose to interpret certain trends in psychology. I have in mind, also, to popularize the approach to behavior thus indicated; alike for the educational procedure and for the social bearings and control of conduct, the neurological view has large significance. An educational campaign to bring it within the sphere of knowledge of those responsible for the guidance of behavior is eminently desirable. My central purpose is to contribute to its psychological foundation

If I am right in this position, the indications of its importance must be many; it is hardly likely to be an individual discovery. As is true of most intellectual movements, the germinal ideas are suspended in the psychological atmosphere; the intention is to precipitate them for the moment in a twenty-minute shower. Theories, principles, interpretations, and a large and varied mass of observations form its support. Within so limited a temporal focus I can hardly hope to bring a wide survey into a single picture, but may suggest the principles of its composition.

To place neurology in the center of the picture is in so far misleading, that we know less of behavior in terms of nerves than of mind; and the latter brand of knowledge is the more extensive, the more illuminating, the more practical. An exact but less acceptable term would be the 'psychobiological' aspect of behavior. 'Neurological' has a popular convenience and currency, and centers attention upon the basic fact that behavior is a matter of nerves; it safeguards thinking about behavior by recognizing a fundamental conditioning. If the neurological bases were adequately known, they would presumably provide formulæ for the differentia-

tion of contrasted and varied types of behavior, normal and abnormal. They would do so in definite established terms, and not in the general, hypothetical, but none the less distinctly valuable manner which alone is available.

The insight and control that may be expected from further knowledge of the neural mechanisms is an uncertain anticipation. Accepting as favorable instances of recent contributions the relation of over-functioning and under-functioning of the endocrine glands to contrasted behavior trends, and the detailed regulation through the autonomic nervous system of the bodily resources in periods of emotional stress, we may well regard the outlook as promising, though the differential neurology of fairly contrasted emotional states is but partially revealed or even suggested. Similarly, the achievements of the directly experimental frontal attack are unquestioned; their lead is peculiarly essential to all sound progress in the study of behavior. But in large measure the formulation of problems for the experimental technique is derived from other though not unrelated interests. The experimental method is in some aspects directed more to answering questions than to asking them; the experimentalist rightly construes his function as including both. Yet with fullest realization of the experimental contributions to the study of behavior, it may still be maintained that behavior, especially the complicated orders of behavior in the higher reaches of human activity, is profitably known through introspective analysis, clinical observation, and shrewd psychological insight. This triumvirate of terms is in some psychological camps under suspicion, and in others even outlawed. This antagonism is open to precisely the same charges of misapplied concepts or of prejudice as is directed by the behaviorists against those whom they egocentrically call 'mentalists.'

The most ambitious claimant of the field of behavior is the behaviorist. If of that type which proclaims dominion by declaration of a strictly objective, stimulus-response, closed-shop platform, over the entire unionized field of behavior, he may as well be faced by a direct reply. The fallacy of such a position is two-fold. The first is that of

overlooking in the stimulus-response equation that the vital factor is neither the initial term nor the final one, neither the button nor the bell, but the nature of the motive force that supplies the bond between them, the battery. For the details of this transgression of the logical code, I refer to Professor Thurstone's lenient exposure of the stimulus error. For the second fallacy, the fallacy of explanation, I cite the recent volume by Associates of Columbia University, who in a laudable attempt to teach the Sophomore how to shoot, frequently shoots over their heads and hits the dons. To a mechanist explaining that 'chemical changes . . . increase the sensitiveness to certain stimuli just as the copepods become slaves to the light when carbon dioxide is added to the water,' they reply by the reminder: 'that human beings dying for ideas and copepods moving toward the light may have certain characteristics in common is, for most purposes, of infinitesimal importance compared with the enormous gulf that will always remain between them.' Or in other words the scientist, in 'reducing' one phenomenon to another by proving that 'hot objects are not really hot, but only vibrating rapidly, or that the sunset is not really a blaze of color, but only complex wave-motions, or that the man is not really manifesting the highest ethical activity but only responding to chemical stimuli, in so far as he allows this notion to creep into his mind, he is very obviously talking nonsense.' The behaviorist's form of this 'reducing' process is not, as he fondly imagines, a getting rid of superfluous psychological tissue, but an arbitrary dietary restriction. The behavioristic concept of behavior is meagre to the point of poverty; its richest nugget is the conditioned reflex contributed by the salivary glands of Pawlow's dog. An edifice of ambitious proportion has been built to enshrine this philosopher's stone. I should have been willing to subscribe to a monument to that dog had he declined to salivate, and thus averted the hemorrhage of ink that followed the operation; while the bell that conditioned his behavior is still regarded by pious devotees to be sounding the death-knell of all accredited psychology. The conditioned reflex is an added behavior

type of obscure and uncertain meaning and range. Just as the discovery that heat is a mode of motion adds new and important facts and concepts, but does not deprive us of the old ones, likewise the behaviorist has added new and important facts to psychology, and has set old ones in newer and truer light; but he has not dispossessed psychology of its authentic acquisitions.

II

The neurological concept of behavior has an elliptical orbit; in the one focus is the standard neural normal process as regulative, in the other the group of functional nervous liabilities to disorder—the abnormal. Assets and liabilities go together; they are of one conditioning, of one nature. Each must be studied closely with reference to the other. Trends of behavior of fundamental significance are revealed under the low as well as the high magnifying power of their abnormal issues; without that clue the meaning of many symptoms in the normal field would be blind. Through the absence or imperfect command of that key, the interpretation of behavior has frequently gone astray. It enters vitally into the formulation of the concept of behavior here advocated. And this brings me to the first of the series of illustrations which I shall employ as the briefest method of presenting my thesis.

(1) Mental disorders are in the first instance clinical pictures. They are likewise nature's demonstrations of the liabilities to defect and distortion of the neural mechanisms, and as such reveal the inherent sources and patterns of behavior. The minor disqualifications, as the more common liabilities, stand closer to the psychologist's interests. Of these, hysteria is by far the most representative. There is clinical hysteria, and there are hysterical varieties of behavior. The fact that the two should be so related that such common departures from normal conduct as defective emotional control; undue susceptibility to the anger response; over-excitability in any of the emotional fields—sympathetic, passionate, or explosive; the too personal reaction to experience; over-conscientiousness; indulgent romancing; episodic lapses from propriety—all capable of expression in lighter

moods and phases; that these should be of one origin with such abnormal phenomena as profound dissociative lapses, fugues, amnesias, trances; with violent motor outbreaks; with pseudo-anæsthesias—hysterical blindness, deafness; with all sorts of paralyses and loss of motor function; with mimetic assumption of ailments and their symptoms; with shrewd deceptions and sensational hyper-suggestibility; this formulation seems an amazing and yet established clinical conclusion. Clearly, only a small percentage of those with hysterical trends and qualifications ever present explicitly the major symptoms, the central manifestations of the clinical picture; and of those who do so, many will succumb only under the stress of an unusual and severe strain or shock; such as illness, tragedy, and—a recent illustration—shell-shock, really the fear-shock of war. To realize the terminal issues of the general and widespread liability to hysterical behavior in the magnified writing on the wall of major hysteria, is to appreciate one of nature's major lessons in psychology.

Interpreting the concept of hysteria for normal relations, I conclude that the hysterical behavior route is one of the most significant of all psychological highways, with many devious by-ways, beset with dangers, which when recognized will be lined by our enterprising pedagogic allies with emblazoned advice, which those who run in motor cars may read. The human race is by original nature hysterical. All children are inherently hysterical. Primitive peoples are in their own type saturated with hysteria. The problem of civilization is to eliminate the hysteria from the human race, to reduce it to controllable proportions, to sublimate it. Standards and cultural levels of behavior may be characterized by their freedom from hysteria. The test of normal maturity is freedom from hysteria. In brief, the hysterical liability supplies the clue to the meaning of basic and crucial behavior symptoms, and affords insight into processes and mechanisms directive in character, emotions and expressions, with versatile educational applications.[1]

[1] At this point I make a detour. Having arrived at these conclusions years ago, through an uncertain groping for a unified inclusive view of the protean varieties of

I can do no more than outline the series of problems centering about hysteria as it contributes to the concept of behavior. There is the dissociative trend: sleep, trance, hypnosis, the true hysterical lapse, dual personalities, and again the narrowing of consciousness and exclusion of fields of perception and areas of response—often psychically selected; clearly this entire group of manifestations must have a common explanation, if the right concept can be found. And when found, it will be represented in a milder temperamental trend, a certain mental attitude or psychic disposition, something having its basis in the neural pattern of response which is shared by the hysterically inclined. That these obtrusive mental symptoms have a deep emotional motivation is a generally accepted view. The Freudians regard the mechanism as imposed or devised from above, while the motive source acts from below, but is denied recognition by a censorial suppression. More likely there are several levels of suppression and repression, with fairly differentiated mechanisms, as there are likewise levels of inhibitions and resistances, presently to be considered and, if possible, also brought into a common formulation.

Next, may be mentioned the hysterical source of energy, to illustrate that these liabilities have their redeeming qualities. There is a certain aggressive, driving, enthusiasm-compelling, if fitful and emotional, energy generated in the seething hysterical complex that helps people to live and brings it about that some who accomplish much do so on an hysterical basis. It is not a wholesome form of energy, though often

hysterical behavior, I received a shock of discovery hardly compensated by the welcome of corroboration, when I found that Dr. Donald E. Core of Manchester (1922) had expressed closely similar views. Dr. Core is a neurologist—a practising alienist with a speculative turn; he has developed a difficult terminology and a system of interpreting 'Functional Nervous Disorders' that in its details is hardly likely to command a following. But he has the larger view of hysteria, and along with it has developed certain fundamental concepts which have psychological value. That he should reach them from the clinical approach is certainly a strong confirmation of the general view here emphasized. That hysteria is inherent in human nature, that its reduction is the problem alike of civilization and education; that it is the great regressive type of disorder; that it is aggravated by unfavorable environmental conditions; that it affords a clue to important neural mechanisms—all this and much more is to be found in Dr. Core's contribution, making it, despite its difficult exposition, a notable one.

the best available. We may also come upon such varied exemplars as the over-conscientious Puritan, the exotic Bohemian, the radical extreme, the gushing sentimentalist, the faddist such as the benign anti-vivisectionist, the front-page specialists, the 'psychics' convinced—though often neither purely nor simply—of their mediumship, and the sincere believers who mistake messages from their subselves for revelations from the beyond. Or again turning to more practical problems, the difficult personalities, the wayward, the delinquent, the maladjusted, the misunderstood, and the great miseducated—in all the hysterical clue to behavior is suggestive. In all these runs a common theme—though variously transposed—and rendered in different keys and tempos.

Returning to the germinal focus of hysteria, the typical emotional lack of control is in the management of the anger response, more typically the anger response of irritability than that of combativeness, petulant tantrums rather than volcanic 'rough-house,' though a little of each. Recent studies of delinquent women emphasize nothing so strongly in the problem of their treatment as the tendency to anger explosion, at times making necessary a riot call as the brawl grows by contagion; and their further subjection to the primitive sway of childish emotion—in all a revealing picture of regression.

(1: *a*) This factor adds to the neurological concept: the hysterical are those who fail to grow up; they retain childish modes of response; they are centrally subject to tantrums as are children. The tantrums change in character and may become nagging, or fault-finding, or just plain vulgar losses of temper, or sullen streaks, or geyser-like eruptions, but are of one origin. It isn't the mental age of hysericals but their *emotional age* that is retarded, and that only in some aspects. Dr. Core thus makes hysteria the great prototype of the regressive neuroses.

In moral vein, any one, whatever his or her successes or achievements measured by other scales, who fails to control his or her anger-responses is a psychic failure. If ac-

cepted, this principle may well redirect the entire emphasis of moral training in the home and the school; furthermore, since the recognition by the subjects of the hysterical meaning of their responses is itself disciplinary, and since—again a point in which I am anticipated by Dr. Core—the environment (which he in turn distinguishes as the environment of the upbringing and that of the mature situation) is largely responsible for the non-correction of the hysteria, educational measures form an integral part of mental hygiene. Hysterical persons should be plainly told that they are such, and their symptoms and tendencies made clear to them not primarily as moral faults but as menacing neural frailties. Such revelations will precipitate much gnashing and wailing, and is a task only for the wisest and strongest of counsellors; for on the one hand, many hysterics cling to and are proud of their hysterical traits, while others will resent with energy the implication that their traits are of such ill-omened quality. But in some such form of psychoanalysis lies the redemption of mankind. I am prepared to support a 'National Association for the Suppression of Anger' as a large enterprise of the American Psychological Corporation, alike for the suppression of private brawls and squabbles, and public clashes of classes and interests, and as an aid to the larger peaceful mindedness that shall make war impossible.

(1: *b*) Prominent in the hysterical contribution to the concept of behavior is the contrast between men and women—the greater and the different liability of the feminine organism and the feminine temperament to the devastating sway of hysteria. My dictionary informs me that such words as shrew, termagant, and similar encomiums were at one time applied to both sexes, and then transferred to one alone—in itself a baleful homily; yet by way of compensation, 'virago' is described as a woman acting like a man. Be this as it may, the concept of behavior here developed makes masculine behavior and feminine behavior as comprehensively and inevitably different as any range of contrast likely to appear above or below the psychological horizon. That this profound differentiation of nature should not be represented in

behavior trends would be an anomaly of a prodigious order. No one but the editors of popular magazines who accept such articles, and the psychologists who write them, would be misled by the fallacy that because men and women do equally well (which usually means equally poorly) in certain college studies, and are not widely apart in tests that measure selected aspects of proficiency remote from any marked biological meaning, men and women are therefore substantially alike, and their differences are largely due to pressure, prejudice, opportunity, and masculine ideals of feminine behavior. The neurological answer is far different and more authentic; it projects the two trends as distinctive in all fundamental and derivative aspects of behavior.

(1: *c*) I must not leave the abnormal clue to behavior without indicating the similar significance of the other leading liabilities of the mind's frailties and disasters. The counterpart of hysteria is neurasthenia; and neurasthenic varieties of behavior enter as instructively into the interpretation of normal behavior trends as do those of hysteria. They form a different and a contrasted tale, pointing a different moral. One must not be misled by the overlapping symptoms of the two, and their common and proper inclusion in the general concept of nervousness, their often joint bearing upon the nature of the neurotic constitution. Dr. Core supports me in the psychological analysis—which is opposed to that given in many of the texts in psychiatry—which places the two disorders on opposite sides of the great divide. Neurasthenia is the comprehensive *progressive*, not regressive liability; its instinct distortion lies in the emotion of fear, as opposed to that of anger. The two disorders and trends share in the hyperthymic symptoms, the greater sensibility to emotional response, but deviate in some directions slightly, in others markedly in their specific manifestations. Neurasthenia is disposed to attack the male, as hysteria the female, by virtue of greater congeniality with the natural expressions of the respective sex-traits. So once again masculine and feminine assets and liabilities diverge; and even when similar achievements result, they are achieved, as behavior is ex-

pressed, upon a different perspective of qualities. And in addition, each one of the great liabilities, the trunk-line insanities, enriches the neurological concept of behavior. In no strained sense there may be recognized a paranoiac, a manic-depressive, an exuberant megalomaniac, a generally psychopathic temperament, presenting behavior patterns within the normal range, allied to the characteristic abnormal expressions that find their clue in the extreme issues.

III

(2) The second great field of illustration of the neurological concept of behavior is in its application to the child. The era of child study came and went, and left along with a small deposit of value a considerable mass of scattered debris out of which skillful professors of education still manage to construct shelters for their doctrines. Yet the time is ripe for the reconstruction of the entire genetic aspect of psychology; the closer, more deeply analytic study of the child is yet to be undertaken. Origins are important; nature is authentic; in the infant the behavior trends are simplified; the earlier levels and types appear, as yet not complicated by their destined transformation. Much of our psychology starts too far along, too high up in level; the behaviorist has done well to set forth the native responses of the infant untainted by the atmosphere of education. Such functions as sleeping and feeding have been too slightly considered; for in their regulation nature gives lessons in elementary psychology. Progress in infancy is so rapid that critical moments must be observed as they occur, or the opportunity lapses. The neurological interpretation is essential; and the symptoms of nervousness in children present unutilized data for insight into mechanisms and the direction of their control. Here, as elsewhere, the bungling management of the educational procedure is due to the use of false or but partially appropriate concepts and to the failure to read child life neurologically, mainly through the all too human predilection for moralizing, also through over-interest in superficial aspects of behavior.

I must limit illustrations to a few symptoms, and first by way of instinct-distortion, the waywardness of the feeding habit. Since it has been recognized that the food summons has two calls—hunger and appetite—with the latter far the more complex, and open to the selection of taste, choice, indulgence, caprice, habit, the psychology of food has received casual but not adequate attention. To the normal child as to the infrahuman animal, feeding is a sufficiently engaging response to continue to satiety; yet the commonest neurosis in children is the apparent absence of any regular desire for food. Observed closely the symptom is not, at least not mainly, a lack of fair appetite, but an aversion to food; the attempt to eat arouses an inhibitory negative response. This aversion is itself the next most common symptom of child-nervousness and extends to all manner of behavior, to the rejection of all proffered pleasures, suggestions, or commands; it is *der Geist der stets verneint*, and is readily mistaken for obstinacy with which it has a true kinship.

It raises a general psychological problem of large import. Inhibition, resistance, shyness, repression, blocking and choking of impulses; what does it all mean? Why are these handicaps of behavior so prevalent, and why and how do they select their fields of operation? That there is a physiological level of the phenomenon, as also several psychological levels, is fairly clear. That the course of true love should not always run smoothly is readily intelligible, for that is a mature and complicated urge; but that so simple and lowly a response as feeding should be open to like complication is rather surprising; and equally is it worth attention that this lowly urge finds its neurotic expression in childhood when that urge is normally keen. It directs attention to the fact that all fundamental urges must be strong to perform their natural functions, and being strong may be overstrong, and likewise subject to arrest and frustration and distortion. At a later period the same neurological liabilities may be traced in the manifestation of the sex urge, or of the combative urge, or of the hoarding urge, or of the assertion of self in the superiority (or Narcissus) complex, and again negatively in the in-

feriority complex. The problem of negativism is characteristic. It appears as a psychopathic symptom in serious mental disorder and may proceed indefinitely to the refusal of food, of all action, petrifying the atonic patient into a lifeless picture of negative inactivity. All this illustrates how the neurological concept of behavior plays up and down the scale of response from infancy to old age, from normal to abnormal, from type to type, and everywhere creates significant problems.

That the food refusal of the child is associated with negativism appears in the fact that the food response is apt to precipitate or be allied with a general negativistic mood. More particularly, the food effort acts as a specific stimulus, a conditioning; if the resistance is overcome, feeding often proceeds for a time quite regularly. That is why appetite comes in eating, and also why sleep induces sleep: once the sleep resistance or food resistance or suggestion resistance yields, the normal reaction ensues. That there is a definite mechanism at work is undoubted; whether at the infant food-level it is connected with the autonomic nervous system, as seems probable, or how it operates then and later is yet to be determined. The right concept plainly has a bearing upon cure and treatment. The neurological treatment is ever one of diplomacy; going around the ends rather than pushing through the center. Nerves must be outwitted rather than fought, safeguarded from too harsh exposure. Thwarting, opposition, disregarding the decrees of nature, insisting that discipline and an arbitrary moralizing shall correct what to the adult is a bad habit, are all wrong from the neurologist's approach.

There is thus indicated a range of problems associated with one of the authentic neurotic symptoms of childhood; nor does the tale end there. It might be continued by citing the habit-spasms of childhood, of which thumb or finger sucking is the most common, often associated with a collateral habit of stroking the ear, or rubbing the nose, or still earlier in infancy fingering a piece of flannel in going to sleep. These infantile tension-relieving habits are equally hall-

marked by nature; they appear when food resistance is 'on,' under fatigue, under excitement; they are often absent in illness when the nervous response is relaxed and at a low ebb, and they may persist through sleep. Their physiology is well worth study, and may reveal the uncertain line between subconcious and conscious functioning, which to Freudians as to others plays an important part in later interpretations.

For lack of time, I cannot follow the neurological psychology of childhood more closely. I must not leave it without pointing out that in this perspective the reëducation of his nervous system is imposed upon man in a far different sense than obtains in any other organism. The true psychological definition of man is the creature who must remake his own nature. He must pass from primitive and infantile modes of behavior to mature ones. He must get hold by conscious regulation of functions that at an earlier stage are adjusted mainly by simpler mechanisms. The strain thus imposed upon early childhood is tremendous, and is not merely the reflex of social pressure which increases with years, but is in truth determined by natural maturing. Here again the control of the anger complex is central, and the childish susceptibility to its sway makes irritability one of the distinctive nervous states of the child. His largest task is the acquisition not of knowledge but of emotional stability, by whose support reason and poise and the life intellectual emerge.

IV

(3) I come next to the Freudian doctrines. They form but one interpretation, a comprehensive one and richly elaborated, of the neurological concept of behavior. According to the faithful, Freudianism is the truth, the whole truth, and nothing but the truth; I question the accuracy of the description. In an address commemorating the twenty-fifth anniversary of this Association, I ventured to characterize it as an important discovery made by the wrong man. I still hold to that opinion. Freudianism has come to stay; it has permeated psychology, affected its entire attitude to-

ward the deeper motives and mechanisms of the vital urges and their derivative expressions. It compensates for the over-intellectualism of the preFreudian psychologizing. The sound part of the system is that which can be brought into harmony with the general view here advocated.

Assuming the tone of prophecy for the sake of brevity, I foresee that the future of Freudianism lies in its application to normal motives and their vital, instinctive-emotional responses, in the hold which it gives upon human traits and character analysis; in this application it affords but one clue to the composite neurological interpretation. The several parts of the Freudian system seem to me loosely integrated, so than an apparently eclectic adherence and rejection may actually proceed by way of a critical interpretation. The glaring sex-cult of the extreme Freudian blinds rather than illuminates. In the war neuroses the danger instinct replaced the sex instinct as the major factor in the neuroses; yet the distinctive emotional reach of sex can have no rival in motivation. No other human plot can replace it in the human drama; but that the incidents are carried in a sex idiom, symbolic or cryptic, is a questionable and even an unimportant conclusion. So, likewise, the parent fixation and the extravagant use of symbolism in dreams and myths—apart from a modest anthropological application—has a limited bearing upon the central significance of the system. All these strained interpretations may be relegated to a subordinate place and leave the rôle of Freudianism in psychology the stronger for such restriction. Nor does the future of Freudianism lie in its psychoanalytic application, which seems to be following the way of all flesh cures—and all mind cures—by running high with the tide of novelty, aided by the special appeal by which one or another cult selects its devotees. The technique of the confessional is as authentic as that of Freud, while the subconscious functionings involved find their most instructive instances quite apart from any Freudian implication. The injection of Freudian motive into such commonplace episodes as the slips, lapses, forgettings, mislayings, crudely ignores the simpler, and

plainer explanations which are more adequate and more natural; the occasional authentic Freudian factors are real enough when one is fortunate in finding one 'good' case in a score. To suppose that people lose things because they have a suppressed grudge against their possession, and then offer liberal rewards for their recovery is to read the 'Lost and Found' columns in a novel manner. The Freudian censor so far as he is not outfitted to play the part will find his place in the problem of suppression elsewhere indicated.

On the side of mechanisms and complexes, the Freudian attack is sound and illminating; far more so because the level of their elaboration is high—as witnessed in their protection by rationalization—than is true of dreams with their lowered condensed psychic tempo, and intermediately of day-dreams; these in turn introduce another valuable factor in the Freudian analysis, the reality principle. It is the 'combination' of the Freudian principles, the wish principle, the pleasure-seeking principle, the pain-repressing principle, the superiority and inferiority principles, the compensation principle, the rationalization principle, the fantasy principle, that prove their worth by opening secret and exposed locks of behavior. The principles mark the focal range in which Freudian concepts work best. In brief, the neoFreudian view will approach and be absorbed in the general neurological view of behavior and make its contributions within that field, not as a rival to it but as a highly significant set of formulations. We shall have fewer distinctively Freudian books and more distinctively neurological ones. The reaction against Freud is due largely to just those aspects of his system that are most questionable, as well as in some quarters most objectionable. With the more critical integration of the underlying concepts, the absorption of the Freudian apperceptive apparatus will be consummated. Freud, despite his defects, has added a dimension, or if not that, a conveyance to psychology—whether a speculative fourth dimension or a needless fifth wheel the future must decide.

V

I have crowded the foreground of my picture with a few large scale figures, leaving scant space in the interesting background for bare suggestive outlines of the neurological scene. It would serve slight purpose to enumerate what I have omitted. I shall be content to return to the practical aspect, and the conclusion that the point of view here presented is in the modern air. That the Freudian interpretation of behavior contains a challenge to the monopoly of moralizing appears in several contributions—that of Holt's 'The Freudian Wish,' one of the earliest and best; that this and allied neurological concepts have their place in the schoolroom is similarly documented, and Watt's little book on 'Abnormal Psychology' as applied to education is a favorable but partial presentation. The provision of special courts for juvenile offenders is a recognition that nerves play as large a part as morals in the stimulations to crime. Psychopathic terms appear in the daily press; kleptomania is no longer considered a high-brow euphumism for stealing. Church clinics to minister to souls in distress bring neurological and spiritual guides together in mutual aid. Social problems, from mob action to war-time prejudice and labor unrest, are in some sense a matter of nerves. The neurological key will be and should be made available to many uses, even at the risk of fumbling in the hands of the inexpert. It is abundantly recognized that human behavior cannot be too preciously safeguarded; that we need for the purpose all the aids that psychology can command. Intelligence quotients are not going to save individual minds or redeem the human race. If so disposed, we may point with pride to the achievements of intelligence in changing the face of the earth, though the latter is often more conspicuous than the constructions. The completing measure of human greatness lies in the control by that same intelligence, of man's own inherent liabilities to unwise behavior. In that consummation a recognition of the neurological insight is destined to play an increasing part.

MEASUREMENT OF INTELLIGENCE

I. The Present Status[1]

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Existing instruments for measuring intellect[2] developed from three roots, the interview, the school examination, and the 'tests' of sensory acuity, memory, attention, and the like, devised during the early history of psychology. The Stanford Binet, for example, is an improved, systematized and standardized interview. The Army Alpha is in part an improved school examination and in part an improved battery of tests like those used before 1900 by Galton, Ebbinghaus, Cattell, Jastrow, and others.

Existing instruments represent enormous improvements over what was available twenty years ago, but three fundamental defects remain. Just what they measure is not known; how far is it proper to add, subtract, multiply, divide, and compute ratios with the measures obtained is not known; just what the measures obtained signify concerning intellect is not known. We may refer to these defects in order as ambiguity in content, arbitrariness in units, and ambiguity in significance.

Ambiguity in Content

If we examine any of the best existing instruments, say the Stanford Binet, the Army Alpha or the National Intelligence Test, we find a series of varied tasks. Some concern words, some concern numbers, some concern space relations, some concern pictures, some concern facts of home life. Some seem merely informational; some are puzzle-like.

[1] This investigation is one of a number on the 'Measurement of Intelligence' made possible by a grant from the Carnegie Corporation to the Division of Psychology of the Institute of Educational Research of Teachers College.

[2] We shall use 'intellect' and 'intelligence' as synonyms throughout this series of articles.

Some concern mental activities which will be entirely familiar to almost all of the individuals to be tested; some concern novelties. Some are irrespective of speed; in some speed is a large element in success. In particular, as we shall see later, the score attained is a composite in variable proportions whereby A is rated as more intelligent than B—first, if he can do certain hard tasks with which B fails, second, if he can do a greater number than B can of tasks of equal difficulty, and third, if he can do more rapidly than B tasks at which both succeed. The only sure statement of what abilities the Army Alpha measures is to show the test itself and its scoring plan.

To this it may be retorted that this variety is not really an ambiguity, that one of these tests is a representative sampling of tasks for intellect, and that the scoring plan is one which weighs each response according to its importance as a symptom of intellect. Unfortunately this is not true. We may cherish the hope that these tests approximate to such representativeness of sampling and suitability of weights. In fact, however, nobody has ever made an inventory of tasks, determined the correlation of each with intellect, selected an adequate battery of them, and found the proper weight to attach to each of these. Such a procedure was carried out in part by the Committee responsible for the construction of the National Intelligence Test, but limitations of time and funds restricted it to a very small fraction of what would be adequate. If anybody did this wisely, a large fraction of his labor would be precisely to find out what abilities our best present instruments did measure, and how these abilities were related to intellect; or to find out what abilities constituted intellect, and how these abilities were measured by our present instruments.[1]

One of the main lines of work in the improvement of instruments for measuring intellect is then to find out what abilities our best present instruments do measure.

[1] The balance of his labor might be expended upon experimentation with tasks that seemed promising as symptoms, even though we did not know what abilities they required.

Arbitrariness of Units

The score obtained by using the instrument to measure an intellect is in present practice either a number representing a summation of credits and penalties or, more rarely, a number representing the grade of difficulty of the tasks which the person can respond to with some assigned percentage of correct responses. Thus in Army Alpha he may score by summation from 0 to 212; in the first suggestion of Binet he could score 5 or 6, or 7, or 8, or 9, according as he was able to do correctly all but one of the tasks set as 5-year tasks, 6-year tasks, 7-year tasks and so on.[1]

In neither case (even supposing the measurement to be a perfect representation of the person's abilities) can the numbers be taken at their face value. If A scores 50 on Alpha, B, 75, and C, 100, we do not know that the difference between A and B in the abilities tested by Alpha is the same as the difference between B and C, nor that C has twice as much of these abilities as A. If D scores mental age 4, E mental age 6, and F mental age 8 by the Binet, we do not know that, in the abilities tested by the Binet, F excels E as much as E excels D, or that F has one and one third times as much of these abilities as E has. The numbers, 1, 2, 3, 4, etc., designating the scores made by individuals, do not represent a series of amounts of intellect progressing by equal steps. The difference in intellect between Army Alpha 10 and Army Alpha 20 may indeed conceivably be as great as the difference between Alpha 100 and Alpha 150. From Stanford Binet 40 months to Binet 60 months may be as great a difference in intellect as from 140 months to 180 months. The value of what is called 1 on the scale is not known, and its value may fluctuate greatly as we move along the scale.

We have then no right to add, subtract, multiply, or divide with these scores of A, B, C, D, E, and F in the way that we do with their heights or weights. Suppose that A

[1] This suggestion was, however, abandoned in favor of a procedure which mixes the two sorts of measure. The procedure is, "Take for point of departure the age at which all tests are passed; and beyond this age count as many fifths of a year as there are tests passed." ['The Development of Intelligence,' Eng. trans. of Kite, 1916, p. 278.]

scores 100; B, 110; C, 90; and D, 120. We cannot say that the average intellect of A and B equals the average intellect of C and D. If E changes from 60 to 70, while F changes from 70 to 80, we cannot say that they have made equal gross gains.

The numbers designating the scores made by individuals are usually not even approximately related to any true zero point.[1] Consequently, even if the scores 1, 2, 3, 4, did represent an equal interval series of amounts or degrees of the ability in question, they would properly be treated as $x + 1$, $x + 2$, $x + 3$, $x + 4$. The 'times as' or ratio judgment is thus not surely applicable and the relations of the scores to anything else are thus undetermined. For example, we cannot say whether the intellect of the average twelve year old is one and a quarter times that of the average six year old or twice it, or ten times it.

The second main problem in improving measurements of intellect is thus to attach fuller and more definite meanings to these credit summations and difficulty levels, and if possible to find their equivalents on absolute scales on which zero will represent just not any of the ability in question, and 1, 2, 3, 4 and so on will represent amounts increasing by a constant difference.

We have to estimate equivalents of this sort somehow before we can make much use of ratings by either credit summations or difficulty levels, before, for example, we can conveniently compare individuals or groups, or the changes made by individuals, or by groups, or the effects of different environments. The commonest method at present is to take as the equivalent for any score by any instrument, the age whose average achievement is that score, and to assume that the increments in average ability are equal for equal differences in age up to some limit such as 192 months, and are zero thereafter. This of course is purely hypothetical in general and is almost certainly in error for the ages near the point where the age change suddenly turns from its full amount to

[1] Attempts have been made to define 'zero' or 'just not any' ability and to assign scores in relation to zero in the case of knowledge of English words, ability to understand sentences, handwriting, drawing, and English composition.

zero. The curve of ability in relation to age is almost always smooth not as in the continuous line, but with a sharp turn as in the dash-line of Fig. 1. The competent thinkers who

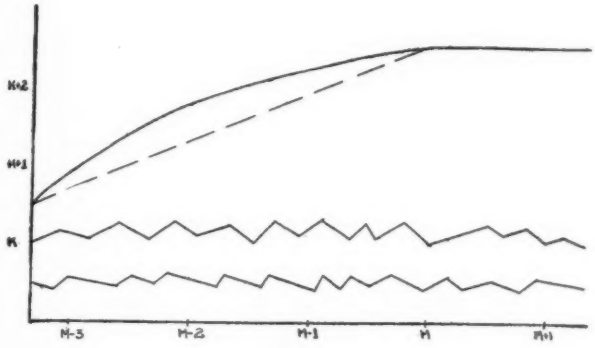

FIG. 1

use the method know this and are cautious in inferences based upon its application to the higher ages; but they use it rather freely for the lower ages, because some method must be used, because it is easy to understand and apply, and because we do not know what method is really right.

It may be objected that equality of units is an unnecessary refinement, for present practical purposes, since the mental age defines the status of an individual sufficiently, 'as able as the average ten year old,' 'as able as the average twelve year old.' These, it may be said, are better measurements for practical purposes than some absolute scale in terms of equal 'mentaces' or 'intels.' The convenience, intelligibility, and realism of the mental age scale up to about 12 or 13 years are indeed great advantages, but after 13 or 14 it is neither convenient nor intelligible nor realistic. It is not convenient because the computation of intelligence quotients becomes very troublesome for the higher ages. It is not readily intelligible because mental ages 14, 15, 16, etc., are *not* 'as good as the average' 14-year old, 15-year old, etc. The average 25-year old for example is about the mental age of 14 by one of the best instruments. It is not realistic

because we have no clear or vivid sense of what the average person is intellectually at fifteen, or at sixteen, and do not even know whether he improves in the next two or three years. A mental age of 15 or 16 or 17 is in fact as arbitrary a quantity as an Alpha ability of 123.

A rarer but more promising procedure than that of transforming test scores into 'ages' is to transform them into units of ability on the assumption that the distribution of ability in all adults 21–30, or in all twelve year olds, or in all pupils in grade six of a certain city, or in some other specified group, is approximately that given by

$$y = \frac{1}{\sigma\sqrt{2\pi}} e^{\frac{-x^2}{2\sigma^2}}$$

For example, the Alpha scores from 0 to 212 were not used in the army at their face value, but were transmuted into seven letter measures by the following scheme, which assumed an approximately 'normal' distribution for a random sampling of 128,747 of the literate white draft. The score

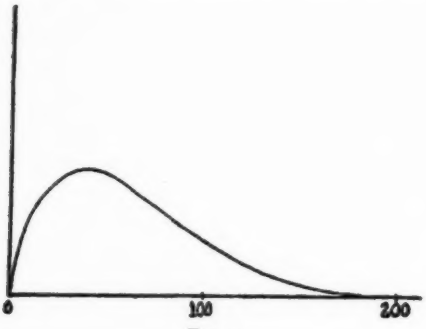

FIG. 2

used in the Thorndike-McCall test of paragraph reading is not the number of correct answers, but a transmutation on the assumption that the real ability concerned is distributed 'normally' amongst twelve-year-olds in American cities.

We know very little concerning the permissibility of the assumption of the so-called normal distribution for adults

or for an age, or for a school grade. The search for evidence pro and con is one important feature of the attempt to obtain units of mental ability which shall be at least approximately equal.

Ambiguity in Significance

The test score measures directly only the measurer's impression from the subject's performance, or the summation in a more or less capricious fashion, of credits and penalties for the subject's responses to the different elements of the tests, or a combination of these. What this score signifies about the subject's intellect depends upon the intuition of the measurer, or upon the correlation between the summation and intellect, or upon both. When we assert that a child is found by measurement with the Stanford Binet to have the intellect of a child of 10½ years, all that is really asserted is that the child does as well in that particular standardized interview as did the average of the children of 10½ years of age tested by Terman in making his standards. We do not know what the average intellect of these children was, nor how closely the Stanford Binet score represents or parallels or signifies it.

When we assert that a man is found by measurement with the Army Alpha to have the intellect of an average recruit in the draft, all that is really asserted is that he does as well in that particular battery of tests scored and summated in a particular way, as the average recruit did. Just what the intellects of recruits were and how closely their Alpha scores paralleled their intellects, we do not know. The measurement is one thing, the inference to intellect is a different thing.

This is of course true of many measurements. The amount of silver deposited in one second by an electric current is not the amount of current. The dividend rate on stock during any one year is not the worth of the stock. The amount of silver is, under proper conditions, of perfect significance as an indicator of the amount of current, since the correlation between it and a perfect criterion of amount of current is perfect. The dividend rate is of very imperfect

significance, since the correlation between it and a perfect criterion of the worth of the stock is far from perfect.

We do not know how closely the rating or score in the Stanford Binet or the Army Alpha or any other instrument correlates with a perfect criterion of intellect, because we do not know what such a criterion is, much less its correlations with these tests. One great task of the measurement of intellect is to obtain such a criterion, or a closer approximation to it than we now have, and to use it to improve the selection and weighting of the elements of our testing instruments.

The present status of such instruments as the Binet or Army or National tests is roughly as follows: We have chosen tests where the judgment of sensible people in general is that correct response or speed of correct response is characteristic of intellect. Such is the case with directions tests, arithmetical problems, common sense questions (as in Alpha 3), and the like. We have chosen tests using the judgment of psychologists in the same way. Such is the case with the completion tests devised by Ebbinghaus, the mixed relations or analogies test devised by Woodworth, and the like. We have tried these or other tests with children secluded in institutions because of imputed intellectual inferiority and with children of like age who are in ordinary schools (as by Norsworthy), with adult males of good health and morals who were found in a Salvation Army home, glad to work for a dollar a day, and with adults of the professional classes (as by Simpson), with children in general of different ages (as by Binet and Terman), with various groups of children ranked for imputed intelligence by teachers, fellow pupils, school advancement, and other symptons (as by Spearman, Burt, Terman, Whipple, Yerkes, and others), with children of alleged superior intelligence in comparison with others (as by Whipple and Terman), with soldiers in the National Guard and regular army in connection with ratings for intelligence given by their officers (as by the Psychology Committee of the National Research Council, with students whose success in high school and college studies was also measured

(as by Colvin, Wood, and many others), with individuals who were tested with a very long series of tests (as by Terman and Chamberlain, Stenquist, and others), and in other ways.

As a general result we know that certain systematized interviews and batteries of tests measure somewhat the same trait, since they correlate somewhat one with another; and that this trait has to some extent the same constitution as the trait which sensible people, psychologists, and teachers rate as intellect.

The failure of perfect correlation between the amount of intellect a person has, as revealed by the criterion, and the amount indicated by the instrument is due, as has been said, partly to the imperfection of the criterion, but partly also to the imperfection of the instruments. They (at least all but one of them) are demonstrably imperfect, since no two of them correspond perfectly in their findings for the same intellects. Since it is extremely unlikely that, out of a dozen instruments devised with about equal care by a dozen individuals or committees at about the same date one should be very much superior to all the others, we may assume, until there appears proof to the contrary, that all are imperfect.

The imperfection may be of two sorts. First the responses measured by the instrument may not be representative of the whole intellect and nothing but intellect; the score obtained may not give enough weight to certain factors or elements of intellect and may give weight to others which really deserve less or even zero weight. The instrument is then like a wattmeter which gives only half weight to the voltage of the current or adds two watts for every time that the current is turned on or turned off. Second, the same person may receive a different score when remeasured by the instrument. In so far as such differences are due to the 'accidental' ups and downs in the person's achievements, they are taken care of by measuring him at enough different times; but in so far as they are due to acquaintance with the instrument itself or with instruments like it, they are a very serious imperfection. For example, a given score with Army Alpha represents a very different

status according as it is from a first, a second, or a third trial. The case here is as if a thermometer tended after subjection to a temperature of 200° once to register 220° when 200° was next encountered. The provision of means for distinguishing between that part of the score due to certain general characteristics of the person measured and that part of the score which is due to certain special training that he has had with the tasks of the tests, or with tasks like them, is thus an important part of the work of making the measurements more fully and exactly significant of intellect.

In general, all our measurements assume that the individual in question tries as hard as he can to make as high a score as possible. None of them can guarantee that the scores would correspond at all with a perfect criterion if the individuals measured tried to appear as dull as they could. The correlation would indeed then probably be inverse, the more intelligent persons being more successful in their efforts to appear dull! It is theoretically possible to arrange a system of incentives such that each person measured by an instrument would put forth approximately his maximum effort, and in scientific testing of the instruments this can often be done. In general practice, however, we rarely know the relation of any person's effort to his possible maximum effort. Since, however, the disturbances due to differences in effort on the part of those tested require in study and treatment procedures which have little or nothing to do with the procedures by which the instruments are made to give better measurements of those who do try their best, we shall disregard the former and shall limit our inquiry to the latter sort of procedures.

Measurements of Intelligence are Measures of Intellectual Products

All scientific measurements of intelligence that we have at present are measures of some product produced by the person or animal in question, or of the way in which some product is produced. A is rated as more intelligent than B because he produces a better product, essay written, answer found, choice made, completion supplied or the like, or pro-

duces an equally good product in a better way, more quickly or by inference rather than by rote memory, or by more ingenious use of the material at hand.

We can conceive of states of affairs such that a man's intellect could be measured without consideration of the products he produces or the ways in which he produces them. Intellect might be exactly proportionate to the activity of the thyroid gland, or to the proportion of the brain weight to body weight, or to the number of associative neurons in the frontal lobes or to the complexity of the fibrillary action of certain neurons, or to the intensity of a certain chemical process, and hence be measurable by observations of the thyroid's action, or estimates of the brain's volume, or by a count or measurement of neurons, or by a chemical analysis.

Psychologists would of course assume that differences in intelligence are due to differences histological or physiological, or both, and would expect these physical bases of intelligence to be measurable. At present, however, we know so little of the neural correlates of intellect that if twenty college freshmen were immolated to this inquiry, ten being the most intellectual of a hundred and ten being the least intellectual of the hundred and their brains were studied in every way by our best neurologists, these could probably not locate sixteen out of the twenty correctly as at top or bottom. Moreover, what we do know of neural correlates is of little avail during life, the living neurons being extremely inaccessible to present methods of observation.

Even if one aimed at discovering the physiological basis of intellect and measuring it in physiological units, one would have to begin by measuring the intellectual products produced by it. For our only means of discovering physiological bases is search for the physiological factors which corresponds to intellectual production.

Measurements of Intelligence Imply Valuation

Our present measurements of intelligence rest on human judgments of value, judgments that product A is 'better' or

'truer' or 'more correct' than product B, that method C is 'preferable' to method D, or that C is 'right' while D is 'wrong,' and the like.

In some cases this is so clear that everyone must admit it. Thus in three of our best tests of intelligence, giving the opposites of words, completing sentences by supplying omitted words, and answering questions about a paragraph read, we make elaborate keys assigning credits to the different responses.[1] These keys are obviously made by human judgments of the value of each response.

The credits given may represent valuations by the truthfulness or wisdom of the answers or sentences, by their grammatical form, by their rhetorical excellence, by their originality, by the rate of producing them, or by a subtle sense of their significance as evidence of intelligence.

In some cases the value is assigned so easily (as a simple deduction from, or following of, a general rule) that we may

[1] For example, the task being to complete,

'God made . . . and . . . let him pass for a man,' we find among the responses of high-school graduates:

him	therefore
him	so
him	then
him	will
him	they
him	he
him	I
him	let
man	always
man	then
man	God
man	has
man	he
man	therefore
man	please
Adam	then
Adam	Eve
Adam	he
animal	wouldn't
Eve	God
us	we
heaven	earth

and must assign some value to each, or make a dividing line between full value and no value somewhere.

thoughtlessly assume that the response indicates intelligence regardless of any process of valuation. For example, we may consider that in a test in arithmetical computation or problem solving, the right answers are signs of intelligence, regardless of what anybody thinks. A little thought will convince us, however, that in such tests the human judgment acts as truly as in a completion or paragraph-reading test. The main difference is that, having once for all decided that right answers are better than wrong answers, we do not raise the issue about any particular answer. We simply assume or make a general rule of valuation. The valuation becomes obvious if we collect all the responses made to an arithmetical task and ask whether all the different 'rights' are equally good or right, and whether all the different 'wrongs' are equally undesirable.[1]

One criterion of value, *truth*, is so widely used in framing, keying, and scoring tests of intelligence that it deserves comment, especially since there may be in the case of truth an objective criterion, power in prediction, by which our judgments of value are or should be determined. Two other criteria of value also need comment because they have been suggested explicitly or implicitly as direct criteria for intelligence. They are *development with age* and *ability to learn.*

Truth

Probably over half of our present tests of intelligence are tests where the response is given credit as a symptom of intelligence in proportion to its truthfulness. Such is the case, for example, with eight out of ten tests of the Otis Advanced; and with Army Alpha, 2, part of 3, 4, 6, 7, and 8. It is more or less the case with Stanford Binet III, 5; IV, 1, 2, 3; V, 1, 2, 3, 4; VI, 1, 2, 3, 5; VII, 1, 2, 5; VIII, 4, 5, 6; IX, 1, 2, 3; X, 1, 2, ; XII, 1, 2, 8; XIV, 1, 2, 3, 5, 6; and with National Intelligence tests A, 1, 3, 4, and B, 1, 2, 3, and 5.

[1] In the special case where we arrange for *Yes* and *No* answers valuation is doubly active. We arrange so that a Yes or a No will be 'good' as a response. Then, since some of the correct 'Yeses' or 'Noes' may be due to chance, and since chance answers are deemed of value, we plan our scoring so as to give the chance 'Yeses' and 'Noes' zero value.

One could make an attractive theory of intelligence and its measurement somewhat as follows: Intellect is concerned with facts, being the ability to see and learn the truth, to get true knowledge and use it to the best advantage. Truth is insight into the real world, the evidence that knowledge is true is its predictive power. Measures of intelligence are then ultimately measures of a man's mastery of prediction, that 2 and 2 will be 4, or that it will be profitable to buy such and such a stock, or that a planet will be found having such and such a path. More immediately, they are measures of certain abilities which contribute to, or accompany, or indicate the existence of, the ability to get and use the truth.

By this theory we should rest our valuations of truth all on the ultimate test of power of prediction. One truth would be better than another in proportion as it predicted more facts, or more important facts, or predicted the same facts more accurately, or helped more in the acquision of other truths. Our valuations of abilities as evidences of intellect would rest on their significance as symptoms of ability to get and use truth.

We may later return to the consideration of this theory and of certain modifications of it. For the present it may be noted that people in general, psychologists, and framers of intelligence tests alike seem to mean by intellect something more than ability in truth-getting to improve prediction. They mean what Pericles and Washington and Gladstone had as well as what Aristotle and Pasteur and Darwin had.

In the oral interview of the business man or physician to test intelligence, in such tests as Ebbinghaus' completions, and in such a battery of tests as Army Beta, there is little obvious reference to prediction or truth getting. In the first case, the aim is rather to see how the person fits his thoughts and acts to little problems or emergencies; in the second, it was rather to give him a chance to use all the so-called higher mental powers; in the third, many tasks were selected in which people who were regarded as intelligent could do better than people regarded as dull, and those of them which most conveniently distinguished the alleged bright from the alleged

dull were kept as the final choice. If these instruments do really measure ability at truth getting, it is only indirectly and more by accident than by design.

It may be that truth-getting is what we unwittingly do measure by our intelligence tests, or what we ought to try to measure, but very few of those who devise or apply the tests think so. And it is surely wise to find out what we do measure before deciding that it is or ought to be truth-getting.

Development with Age

Binet had it in mind to discover those intellectual abilities which six-year-olds had that five-year-olds did not have, those which seven-year-olds had that five-year-olds and six-year-olds did not have, and so on. It might seem that, except for the one judgment that abilities were 'better' or represented 'greater intelligence' the later they came in this series of normal chronological process, the Binet measurement would be free from valuation.

However, valuation came in from the start because Binet tried only abilities which he valued as intellectual. He did not take *all* the psychological features of five, six, and seven-year-olds and choose as his series of tests those which separated the ages most distinctly. In revising Binet's series Terman and others have paid less and less attention to lateness of development and more and more to significance as valued symptoms of intelligence in their choice of tasks.

This is well. For if Binet or they had collected a series of tasks such as showed the least overlapping of one chronological age on the next, the resulting series would be inferior as a measure of intellect to the series as it stands. For example, quality of handwriting, rate of tapping, and ability in checking A's on a mixed sheet of capitals would probably show less overlapping with age than vocabulary, rate of reading, and ability in completing sentences. But they would be far less effective in diagnosing amount of intelligence.

Development with age would be a poor and partial criterion for intellect of any sort or degree, and for the higher ranges of intellect, say those above the 70-percentile intellect of the

average of the white draft, or above the average ninth-grade pupil, it would be well-nigh worthless. It has never been so used. The Terman mental ages above 14, for example, are not functions of development with age, but of differences between individuals, regardless of age.

Ability to Learn

An obvious hypothesis, often advanced is that intellect is the ability to learn, and that our estimates of it are or should be estimates of ability to learn. To be able to learn harder things or to be able to learn the same thing more quickly would then be the single basis of valuation. Success in solving arithmetical problems, or defining words, or completing sentences would then be good, simply and solely because it signified that the person had greater ability to learn.

If greater ability to learn means in part ability to learn harder things, we have excluded the vague general valuation of certain products and ways of producing only to include it again. For we shall find ourselves selecting or defining A as harder to learn than B on the ground that only the more intellectual persons can do it, or on the ground that it requires a higher type of intellect, and shall find ourselves using those vague general valuations to pick the persons or describe the type of intellect required.

If greater ability to learn means only the ability to learn more things or to learn the same thing more quickly, we have a view that has certain advantages of clearness and approximate fitness to many facts. Even less than in the case of truth-getting, however, do our present actual instruments for measuring intelligence measure directly a person's ability to learn more things than another person can, or to learn the same things more quickly. The substitution test included in Army Beta, in the National Intelligence Examination and in some others, is about the only test of speed of learning that is used; and it is more than a learning test.

Much evidence will therefore be required before we can wisely replace our present multifarious empirical valuations by the formula that intellect is the ability to learn more things or to learn the same thing more quickly.

The reduction of all valuations of response to valuation as symptoms of ability to learn more and more quickly thus seems too narrow a view. It has other defects. Were it true, we ought, other things being equal, to get better correlations with a criterion of intellect from tests in learning something new and from tests deliberately framed to measure how much one has learned in life so far, than from the existing batteries of miscellaneous tasks.

This does not seem to be the case. Quantitative data concerning individual differences in learning under experimental conditions are rather scanty, and their correlations with a criterion of intellect are scantier still; but what facts I have been able to gather do not show that, per hour of time spent, tests in learning predict the criterion as well as as the tests now in use. Tests framed to measure how much one has learned in life so far, such as vocabulary tests, information tests, or such Binet elements as 'Knows whether he is a boy or a girl,' and the like, are valuable, but not, so far as I can determine, so valuable as a composite containing also tests primarily of selective, relational, generalizing, and organizing abilities.

Other Attempted Simplifications of the Process of Valuation

Response to Novelty

In one way allied to the doctrine just described and in one way sharply contrasted with it, is the doctrine that a person's intellect is measured by his ability to respond well to new situations, to do 'originals.' The importance of some such ability as this will, of course, be admitted. However, in view of the great difficulty of deciding just what situations are 'new' for any given individual; in view of the fact that 'to respond well' is likely to bring in many or all of our vague general valuations again; in view of the fact that distinctions among novel situations as 'harder' or of greater demands on intellect will have the same effect; and in view of the fact that our most approved present instruments include many tasks which seem as fittingly called responses to the familiar

as to the new—in view of all this it seems best at present not to try to narrow our valuations to fit this theory.

Relational Thinking

Spearman[1] has argued that intellect equals the apprehension of experience, the eduction of relations and the eduction of correlates. The two processes are defined as follows: "The mentally presenting of any two or more characters (simple or complex) tends to evoke immediately a knowing of relation between them." [23, p. 63]. "The presenting of any character together with any relation tends to evoke immediately a knowing of the correlative character." [23, p. 91].

There is no doubt that the appreciation and management of relations is a very important feature of intellect, by any reasonable definition thereof. Yet it seems hazardous and undesirable to assume that the perception and use of relations is all of intellect. In practice, tests in paragraph reading, in information, and in range of vocabulary, seem to signify intellect almost as well as the opposites and mixed relations tests. In theory, analysis (thinking things into their elements) selection (choosing the suitable elements or aspects or relations), and organizing (managing many associative trends so that each is given due weight in view of the purpose of one's thought), seem to be as deserving of consideration as the perception and use of relations. Moreover, I fear that, in all four cases, we need other valuations to decide which are the *better* relations or *more abstract* relations, or the *more essential* elements, or the *more sagacious* selection, or the *more consistent* organization, or the *more desirable* balance of weights, and the like.

However this may be, our present tests of intelligence are not merely instruments to measure how little stimulus is required to produce a perception of a relation, or how many relations will be perceived from a given constant stimulus, or how quickly. And we may best study them as they are before dismissing the valuations on which they are based, in favor of any simpler and more objective system.

[1] Spearman, C., 'The Nature of Intelligence and the Principles of Cognition.'

We shall then accept for the present the status of measurements of intellect as measures of different products produced by human beings or of different ways taken by them to produce the same product, each of these products and ways having value attached to it as an indication of intellect by a somewhat vague body of opinion whether popular or scientific.

The Content or Data of Tests of Intellect

Presumably a man can use intellect and display the amount of it which he possesses in operations with any sort of material object, any living plant or animal, including himself, any quality or relation that exists in reality or in imagination, any idea or emotion or act. Our tests might draw upon anything for their material.

They have, in fact, greatly favored words, numbers, space-forms, and pictures, neglecting three-dimensional objects and situations containing other human beings. How far this has been due to convenience, and how far intellect is really best measured by its operations with words, numbers, space-forms, and pictures, is a matter that obviously deserves investigation. Our choices of test material have certainly been somewhat determined by convenience. They have also favored ideas, general notions, abstractions, symbols and relations, to the relative neglect of percepts and particulars. This has been in the main deliberate, our general scheme of valuation attaching on the whole more intellectual worth to operations with generals and facts in relations than to particulars and facts in isolation.

The nature and extent of the specialization of intellect, according to the content or material operated on, has been and still is a matter of dispute; and the difference of opinion carries over into the practice of measurement. Some psychologists would be fairly well satisfied to measure intellect by a series of mazes alone; or by a series of sentence completions alone. Others, the great majority, attach much more confidence to a battery of tests including surely both words and numbers, probably also some space-forms

and perhaps some more concrete pictorial material. We shall later present facts, both old and new concerning specialization.

The Form of Tests of Intellect

Whether we consider the external appearance of the tasks or the internal nature of the processes in the person doing them, there is a great variety in respect to form, that is, to the operations performed with the words, numbers, pictures, and other content. Externally, there appear questions to be answered, sentences or pictures to be completed, errors to be found and corrected, definitions to be given and to be chosen, items to be matched, directions to be followed, disarranged parts to be put together, disarranged events to be put in proper sequence, keys or codes to be learned, true statements to be distinguished from false, items to be checked as fit by various criteria, items to be crossed out as unfit, and so on.

Internally, the individual finds himself striving to attend to certain matters, to fix others in memory, to recall what he knows about others, to select from many things or ideas the one which best satisfies certain requirements, to define the relation between two terms, to discover an element common to three or four given facts, to hold in mind many different facts and use them to some specified purpose, and to inhibit customary habits in view of some rule. He also finds himself in some cases (such as many elements of information tests, vocabulary tests, and arithmetical computations) utilizing a wide range of knowledge and skills.

Any system of units of measurement that is to be adequate must then apparently be flexible enough to apply to a wide variety of operations such as we may call attention, retention, recall, recognition, selective and relational thinking, abstraction, generalization, organization, inductive and deductive reasoning, together with learning and knowledge in general.

Scoring the Products of Intellect

In the great majority of instruments for measuring intellect the score or rating is determined in part by the degree of

difficulty of the tasks the individual can do successfully. Thus 'There are three main differences between a president and a king; what are they?' (Stanford Binet XIV, 3) is harder than 'Are you a little boy or a little girl?' (Stanford Binet, III, 4). To complete 3 6 8 16 18 36 . . . . . (Alpha 6, 20) is harder than to complete 10 15 20 25 30 35 . . . . . (Alpha 6, 2). Psychologists and scientific and sensible people in general readily rank tasks as easy or hard for intellect and would accept the principle that, other things being equal, the harder the tasks a person can master the greater is his intelligence. The concept of hardness or difficulty in intellectual tasks, as now used, is somewhat vague and variable. Its outstanding characteristic is that among a large group of persons varying in intelligence, the harder the task, the fewer will be the persons who can do it, and the more intelligent they will be. Sometimes, however, tasks are called hard which really are only recondite, familiar to few; and sometimes tasks are called hard which really are only long.

We shall presently define this concept of the intellectual difficulty of a task, so as to make it more useful in science, but for the present we may leave it vague, the principle stated above being true for any reasonable definitions of 'difficulty,' and 'intelligence.'

In many of the instruments for measuring intellect there are tasks which are of equal difficulty (or at least tasks so nearly equal that which of them is hardest is not certain). In the Binet series the tasks for any one year of age were supposed to be equally hard. In Alpha 7 only by statistical inquiry could one decide which of these is hardest, which next hardest and so on.

6	love—hatred : : friend—*lover mother need enemy*	6
7	wrist—bracelet : : neck—*collar leg foot giraffe*	7
8	sailor—navy : : soldier—*gun private army fight*	8
9	carpenter—house : : shoemaker—*hatmaker wax shoe leather*	9
10	shoestring—shoe : : button—*coat catch bell hook*	10
11	quinine—bitter : : sugar—*cane sweet salt beets*	11
12	tiger—wild : : cat—*dog mouse tame pig*	12
13	legs—man : : wheels—*spokes carriage go tire*	13
14	north—south : : east—*north west south east*	14
15	feather—float : : rock—*ages hill sink break*	15

16 grass—cattle : : bread—*man butter water bones* 16
17 fin—fish : : wing—*feather air bird sail* 17
18 paper—wall : : carpet—*tack grass sweep floor* 18
19 food—man : : fuel—*engine burn coal wood* 19
20 sled—runner : : buggy—*horse carriage harness wheel* 20

21 poison—death : : food—*eat bird life bad* 21
22 Japanese—Japan : : Chinese—*Russia China Japanese pigtail* 22
23 angels—heaven : : men—*earth women boys Paradise* 23
24 Washington—Adams : : first—*contrast best second last* 24
25 prince—princess : : king—*palace queen president kingdom* 25

Now if a test includes a dozen tasks absolutely equal in difficulty for people in general, any one person who gets some right will by no means always get them all right, and any one person who gets some wrong will by no means always get them all wrong. So a person's score is partly determined by the number of tasks of equal difficulty that he does. We must then consider as a possible principle 'other things being equal, the greater number of tasks of equal difficulty that a persons masters, the greater is his intelligence.' This principle would not be accepted so readily as the principle about greater difficulty, and perhaps would not be accepted at all unanimously. 'Knowing more things than someone else, and being able to do more things than someone else' is not so clearly and surely having more intelligence as 'being able to do harder things than he can do.'

The two things have been somewhat confused in general discussions and in the construction of measuring instruments because, by and large, a person increases the number of things he can do in large part by adding on harder ones, and also because the person who can do the harder can on the average learn those which the duller person can learn more quickly than he, and so learns more of them. Consequently what we may call the *level* or *height* or *altitude* of intellect and what we may call its *extent* or *range* or *area at the same level* are correlated and either one is an indicator of the other. It will be best, however, to keep them separate in our thinking.

In many of the instruments for measuring intellect a person's score is determined partly by the speed with which he can do the tasks. Even in batteries of tests where all

candidates attempt all the tasks, speed may count, since the persons who do the easier tasks more quickly may have time to review some of the tasks and perfect their work. If speed deserves any weight in determining the measures of intellect it is by virtue of the principle that, 'Other things being equal, the more quickly a person produces the correct response, the greater is his intelligence.' Giving much weight to speed arouses decided objections in the laity and among some psychologists, and the principle just stated certainly would not be accepted as axiomatic. By and large, however, if A can do harder things than B can, A will do those things which B can do more quickly than B can. A certain moderate weight attached to speed will not then much decrease a test's significance; and, per hour of time spent on testing and scoring, an even greater significance may perhaps be obtained by giving a liberal weight for speed than by giving none.

For the practical purposes of estimating intellect, a battery of tests in which *level*, *extent*, and *speed* combine in unknown amounts to produce the score may be very useful. For rigorous measurements, however, it seems desirable to treat these three factors separately, and to know the exact amount of weight given to each when we combine them.

We shall try to make the concepts of intellectual product, difficulty of producing an intellectual product, range of products produced, and speed of producing a product, more definite and precise, but without so altering them as to lose the elements which have given them practical value in the best current practice in measurement, or to weaken in any way their usefulness in measuring intellects as we actually find them by the tests which we have so far developed.

We shall start with certain first approximations. For a first approximation, let intellect be defined as that quality of mind (or brain or behavior if one prefers) in respect to which Aristotle, Plato, Thucydides, and the like, differed most from Athenian idiots of their day, or in respect to which the lawyers, physicians, scientists, scholars, and editors of reputed greatest ability at constant age, say a dozen of each, differ most from idiots of that age in our asylums.

Let an intellectual product, *i.e.* a product or response requiring, or depending on, intellect for its production be defined as a product or response which, given the same external situation, the intellects in the half toward Aristotle are more likely to make than the intellects in the half toward the idiot. For example, if, when all Athenians of age forty were confronted by the question 'Is a straight line the shortest distance between two points?' the growth of the white blood corpuscles was equal for the Aristotelian and the idiotic halves, whereas the answer Yes was more prevalent in the Aristotelian half, we should rate the latter as a product depending on intellect, and the former as a product not depending on intellect.

Let the intellectual difficulty of producing a given intellectual product in response to a given external situation be defined as follows: Enough time being allowed for production so that an increase in time would not increase the number producing it, the difficulty for Athenians of forty, is approximately greater the smaller the number of them who produce it, provided that the ranking of those who do produce it differs from the ranking of those who do not by greater nearness to the Aristotelian end. We could be much more rigid here by supposing a population to vary from the idiot to the Aristotles in amount of intellect only, being identical in all else. Then, if all conceivable productions of intellectual products in response to given external situations were ranked for difficulty, the order would be very closely that of rarity and of the nearness to Aristotle of those who achieved it. We could omit the 'approximately' and the proviso. Our definition has deliberately been left loose, since we do not know exactly what it is in which Aristotle differs most from the idiot, much less can we know in the case of any group of actual individuals that they are identical in all else than it.

The range of products produced at any one level, *i.e.* of products which are equally hard to produce, is defined simply by their number. What we may call the relative range at any level may be defined as the percent or fraction of the products at that level which can be produced by the intellect in question. The speed of producing any given product is defined, of course, by the time required.

It will be convenient to use the word *task* to mean the production of a given product in response to a given external situation, and to speak of the difficulty of tasks, the number of tasks of a given difficulty that can be done, and the speed of doing a given task.

We now have intellect defined by a ranking of men whose differences therein are roughly appreciated as we appreciate the differences of the world's varied objects in volume (only much more roughly). We have intellectual tasks and products defined in a catholic way that would, for instance, probably include every task in all the stock instruments in use by psychologists today. We have *difficulty* defined objectively so that a series of tasks could be approximately ranked as to their respective amounts of difficulty for any subject specified group.

If we list all tasks, find the difficulty of each, apply an intellect to them, observe which it can do, and how long it requires to do each, we have measured how hard tasks it can do, how many it can do at each level, and how quickly it can do them. If we use in place of a complete list of tasks a fair sampling from them, we have attained the same end, subject to the error of our sampling.

The new problems of theory and technique in the measurement of intellect, that is, the problems not soluble by the general methods of measurement in any science, concern the measurement of difficulty of task. Extent and speed are measurable in two of the most perfect units there are—number and time. In the case of difficulty, however, we have so far provided only for an inventory of intellectual tasks and their arrangement in an *order* of difficulty.

Their differences in *amount* of difficulty and the differences between the amount of difficulty of any one of them and some zero point of difficulty (some task which is just below a task of infinitesimal difficulty), are not determined. To find ways of determining these will be our main work.

Before attempting it, however, we may best consider certain further facts about difficulty, extent and speed in the production of intellectual products, and certain consequences

of our analysis of a measurement of intellect into this threefold determination.

Further Facts Concerning Difficulty

We have defined intellectual difficulty in relation to a defined group of individuals. How far the rank order for difficulty obtained in the case of one human group will hold for others, or for a group of dogs or of chickens, is a matter better ascertained by experiment than prejudged. Difficulty in our treatment is always difficulty for some specified group of intellects, such as our Athenians aged forty. We can, if we wish, specify the group as all human beings of all ages, or all animals, and so get measurements of somethimg which we might call *difficulty in general.* The value of such a measure will, however, depend largely on the closeness of correspondence between the rank orders for the same series of tasks at different ages, in different civilizations, and so forth. If these are very low, the measurement of such difficulty in general may be of very little use.

Many cases of grouping, as by age, by amounts of general education, by amounts of special education, or by city and country environment, are of great importance. Two may be considered briefly now as samples, namely, grouping of those of equal chronological age by amounts of intellect, and grouping of those of equal intellect by chronological age. If certain tasks are of difficulty k, $k + a$, $k + a + b$, $k + a + b + c$, etc., for 12-year-olds of low or small intellects, say the bottom tenth of twelve-year-olds, how far will they retain the same relations in respect of difficulty in the case of the top tenth? If certain tasks are of difficulty k_1, $k_1 + a$, $k_1 + a + b$, etc., for the eight-year-olds of a certain degree of intellect how far will they retain the same difficulty relations, for sixteen-year-olds of the same degree of intellect?

We have eliminated speed entirely from influence upon the measurement of difficulty, by our condition that such a time allowance be given for the task that no further increase in time would alter the production. In practice, this would only be approximated. Obviously we must not make the

time so long that during it the intellect in question changes appreciably by growth or training. We should not leave individuals to strive for ten hours to complete: 'The body .....gives light.....the.....is the sun,' because once in ten thousand times, some child who failed during nine hours succeeded in the tenth. This would be a valuable experiment, but we have far more valuable ways of using ten hours of his time.

What we are really concerned about is to avoid rating one task as harder than another merely because it is longer, so that the poorer intellects do it less quickly than the others, and so, within a too short time limit, show a spuriously greater percentage of failures.

We have made the requirement that the intellectual ranking of those who do produce the response shall be higher than that of those who fail. Usually this requirement is unnecessary. It can, that is, usually be assumed that the good or correct response will be obtained by the better intellects more often than by the poorer. It is inserted to provide against cases where the better intellects are subject to some constant error so that they give fewer correct responses than the dull do, or where other factors than intellect distort the percent of rights from what it would be if everything but intellect were equalized. For example, it is conceivable that, if (*a*) and (*b*) below were given to a random sampling of intellects,

Underline the right answers:

(*a*) 4^{-1} equals $\frac{1}{4}$ 3 5 41

(*b*) $4^{1/2}$ equals 2 3-1/2 8 412

ratings for difficulty by the percents correct would be very much in error. The percent for (*a*) would probably be lower than for (*b*) because, lacking knowledge of exponents, the more intelligent one was, the more likely one would be to report 3 for (*a*), (valid if 4^{-1} means $4 - 1$), and to report 2 (valid if $4^{1/2}$ means 4 halves or $4 \times \frac{1}{2}$) for (*b*).

We have treated the task as to produce a certain product, scored, consequently, as done or not done, success or failure, right or wrong. Now when any task for intellect is set,

there are often many different responses varying in 'goodness' or correctness. In such cases, our method requires that in determining the difficulty of the task, a dividing line be set somewhere.[1] Our method will not, however, prevent us from later using different credit values in a scoring plan for such a task and taking full advantage of whatever added value these more detailed credit values may have in estimating an individual's intellect.

It may be noted further that a task may consist of various combinations and complications of other tasks. Thus the task may be to get the right answer to $8 + 3$, or to get the the right answer to $11 + 7$, or to get the right answers to *both* $8 + 3$ and $11 + 7$, or to get the right answers to $8 + 3$ and $11 + 7$ and also $18 + 4$, or to get the right answer to:

Find the sum
9
4
7
3
8
—

which ordinarily involves the above, plus knowledge of $22 + 9$, of certain words and procedures, and plus control over certain habits, such as holding numbers in mind, and adding a seen to a thought-of-number.

We are now in a position to state one theorem of the measurement of intellect. Let difficulty be defined as above, then:

Theorem 1: Other things being equal, if intellect A can do correctly all the tasks that intellect B can do save one, and in place of that one can do one that is harder than it, intellect A has the higher level.

One is tempted to go farther and assume that, other things being equal, if A and B can do correctly the same number of tasks, A has the higher level, if the average difficulty of the tasks he can do is greater than the average difficulty of the tasks B can do. This cannot, as yet, be wisely assumed first because we do not know that we have any

[1] What seems to be one task to the person tested may be used as two or more tasks by scoring it first with the dividing line at one place, and second with the dividing line at another.

right to average measures of difficulty,[1] and secondly, because, even if we could, it is not safe to assume that as much intellect is required to do 10 tests each of difficulty 20 as to do one task of difficulty 200.

On the other hand one is tempted to suggest the measurement of an intellect by the hardest things it can do, assuming that since it can do these, it could do all easier, as we assume that one who can jump over a bar 6 feet high could surely jump over bars at 5 ft. 10 in., 5 ft. 8 in., and so on. The possible variety and specialization of intellectual tasks makes this uncertain. We shall consider the facts later.

Quantity or Extent or Range

Our definition of greater difficulty enables us also to define equal difficulty and so to make a fairly rigorous definition of quantity or extent or range by making it separately at each level of difficulty. For any specific group G and any specific time t those tasks are equally difficult which are done correctly by equal percentages of intellects.

Consider then all the tasks which are of a certain difficulty D. Some intellects will fail with all of them. Among the intellects which succeed with some of them we may make comparisons according to the number succeeded with. Such a statement as 'N tasks of equal difficulty, D, being given, with t time allowed per task, A did $0.1N$ while B did $0.2N$ and C did $0.3N$,' is clear and useful. We can say that B did twice as many as A, that C exceeded B in the number done as much as B exceeded A, and that the average for A and C was the same quantity as the score for B. Where the problem concerns the extent of an ability, as in the number of certain facts that are known in history or science or the number of certain procedures that are mastered in arithmetic or carpentry, it is often, perhaps usually, desirable to free the measurement from differences in difficulty by making the tasks equal in difficulty and measuring extent at that level.[2]

[1] We have provided for determinations of which one of two or more tasks is the more difficult, but not, as yet, for determinations of how much more difficult it is.

[2] It should be noted that a number of tasks of equal difficulty may be given in a test instrument, not with any intention of measuring extent of intellect at that level

In the measurement of intellect, measurements of extent at each level are obviously instructive for many purposes. The inventory of what intellect A can do is improved by being classified into Tasks 1, 2, and 4 at level D_1, 16, 19, 27, and 28 at level D_2, 37, 43, 48, 49, and 56 at level D_3, and so on.

We can set down as *Theorem II:* Other things being equal if A can do correctly all the tasks that B can do, and can also do one more task at the level of any of the others intellect A has a greater range than intellect B has. We could also safely say that A is a better or more useful intellect; whether we can rightly say that it is *greater* than B is more doubtful. The latter seems to imply that superiority in extent can be made commensurate with, weighed in the balance against superiority in level. For the present let us leave the question open.

Speed

There is of course no essential difficulty in measuring the time required for intellect A to produce a certain product. Number and time figure in mental measurements as they do in physical measurements. The units of number and time are indeed so much more convenient and intelligible than units of difficulty that there is a strong natural tendency in those who devise instruments for measuring intellect to let their measurement depend largely upon the number of correct responses and the speed of producing them.

In the instruments that are actually used, it is customary to have the time a mixture of (1) the time spent in doing some tasks correctly (2) the time spent in doing other tasks incorrectly and (3) the time spent in inspecting other tasks and deciding not to attempt them. This confusion may be permissible, or even advantageous, in the practical work of obtaining a rough measure of intellect at a small expense of time and labor and skill, but for theory at present and for possible improvement of practice in the future we need to separate the speed of success from the speed of failures.

for its own sake, but simply in order to obtain a more accurate measure of the level itself. For example suppose that in instrument x we have tasks at ten levels $D_1 D_2 D_3 \ldots D_{10}$, one at each. Suppose that in instrument y we have ten at each level. Then by whatever convention we determine how hard a task a person can do, we shall determine it much more exactly by instrument y than by instrument x.

To the number of tasks correctly done at each level we may add a record of the time for each or of the average time for all at that level.

Since to save time in intellectual production is a 'good,' we may frame *Theorem III* as follows: Other things being equal, if intellect A can do at each level the same number of tasks as intellect B, but in a less time, intellect A is better. To avoid any appearance of assuming that speed is commensurate with level or with extent, we may replace 'better' by 'quicker.'

Relative Importance of Level, Extent, and Quickness of Intellect

Each of these three factors is essential. If it required an infinite time per task, an intellect would produce no product at any level no matter how high its potentialities as to level and extent might be. If it had zero extent at all levels, it would not matter how high its potentialities as to level or how quickly it could do nothing. In the ordinary sense of the word, however, level is by far the most important. The chief evidence for this is that it alone is indispensable, irreplaceable by anything save itself. If the best available intellect can do only things of level D_{19}, we cannot get things of level D_{20} done at all. If the best available intellect can do only 72 things at level D_6 and we need to get 144 things at that level done, we need only to get other intellects at work, say one that can do 45 of the balance and another that can do the remaining 27. If the best available intellect can do only 10 tasks per minute at level D_3 and we need to get 20 done per minute, we can hire five common people who can do two a minute. Indeed, we shall be wise to hire ten common people to do two a minute each, and leave the best available intellect to put its time on tasks far above level D_3.

Common sense recognizes the greater importance of level. It rates a Pasteur far above the most widely competent general practitioner. It does not ask how quickly Milton could give opposites, or turn out doggerel rhymes. Probably Pasteur was very much above the average in extent of in-

tellect; probably Milton could have written as good poetry as A can write and much faster than B can. But common sense considers extent and quickness as unimportant in comparison with reaching a level far above the average.

From the economic and philanthropic points of view, level is enormously more important. If an intellect could be hired from Mars of so high level that it could learn how to prevent war as easily as Jenner learned how to prevent smallpox, a million dollars a day would be a cheap wage for the earth to pay him.

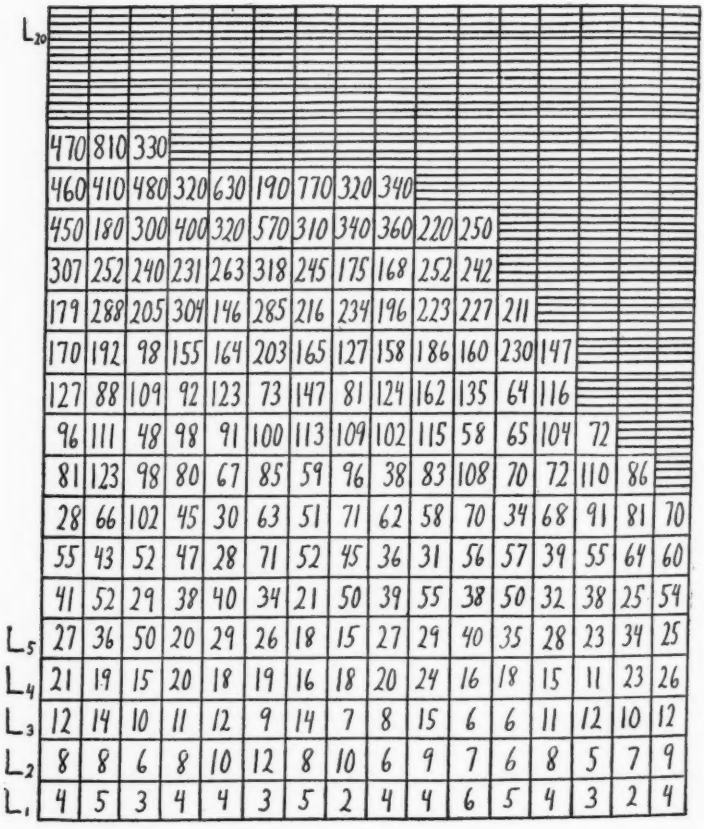

FIG. 3

Our analysis of the measurement of intelligence may be represented by space and number as follows:

Let one sixteenth of a square inch represent one intellectual task. Let those equal in difficulty be placed in the same row across the page; let the order of the rows from the bottom to the top of the page be the increasing order of difficulty; let the square be shaded if the individual in question cannot do it; if he can do it, let it bear a number representing the time he requires to do it. For illustration, we have assumed

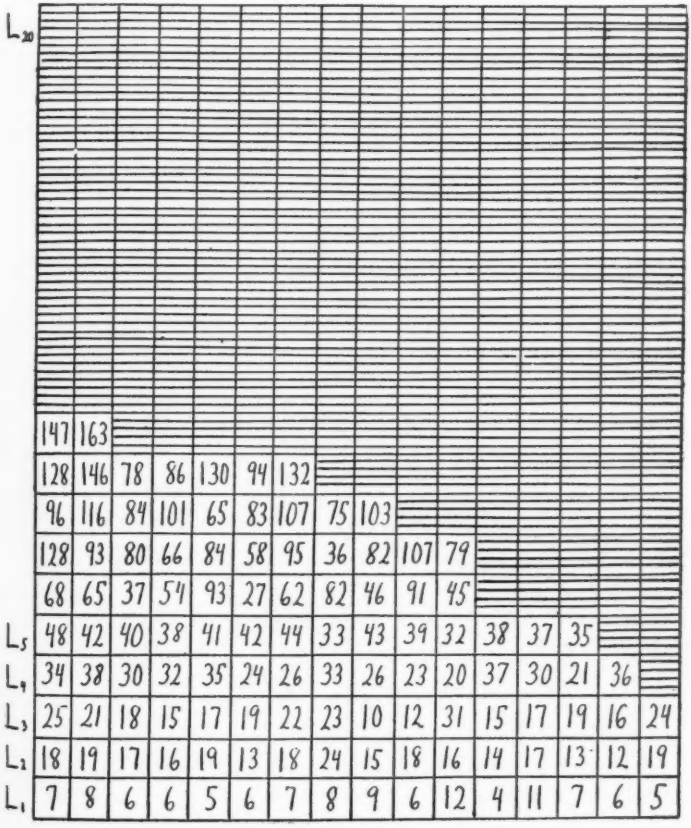

FIG. 4

that there are 320 tasks and that they are of 20 levels of difficulty, 16 at each level.

Figures 3 and 4 then represent the measurements or inventories of two specimens of intellect. Such measurements or inventories may be abbreviated by using a random sampling of tasks at each level, or by using only every other level or every third or every fourth level, or in other ways. Only one thing is needed to make such measurements submissible to the arithmetic and calculus of science in general. That is the expression of the altitude of each level (now merely a rank) as an *amount* of difference from the altitude of the others and from some group of tasks which require intellect but so little of it that they border on a true zero of difficulty which may be set as their lower limit. This is the fundamental problem of mental measurements.

The next articles in this series provide what seems to us a satisfactory solution of this problem.

DISCUSSION

PURPOSIVE PSYCHOLOGY AND THE CONDITIONED REFLEX

BY HULSEY CASON

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Professor McDougall's 'Outline of Psychology' contains the first very aggressive criticism of the conditioned reflex, which is said to be largely responsible for the present mechanistic point of view in psychology. If we can dispose of conditioned reflexes, this writer believes, there will be no hope or support left for behaviorism and its accompanying evils. He claims that "All that used to be called profiting by experience, or intelligent adaptation of behavior, or the acquirement of habits, in short, all facts that used to be classed under the terms 'memory' and 'habit' we are now to regard as essentially a matter of the establishment of 'conditioned reflexes'" (p. 25). It is further stated that if a young student joins the ranks of the behaviorists, "He will be told that the conduct or behavior of any man is merely the sum of his conditioned reflexes; that this is all we know and all we need to know" (p. 26).

However, we may suggest, in answer to some of these criticisms, that no one has made any such claim as this, and certainly no behaviorist has ever denied that anatomical features and reflex tendencies can be inherited, although several writers have denied the potency of instincts, with their affective, conative, and teleological aspects. Professor McDougall also should not denounce the position of some behaviorists in this regard on *moral* grounds, since the discovery of facts which give some hope of changing man and his environment, and at the same time furnish more evidence against the power of hereditary forces and a more or less blind fate, should be an occasion of rejoicing rather than one of despair. We feel that the moral issue involved should prompt one to be very certain of the facts before teaching college students that their lives are mainly shaped by instinct, and that the habits which they may acquire are a secondary consideration.

In discussing Pawlow's proverbial dog, Professor McDougall writes as follows: "If it could be shown that a 'conditioned reflex'

of this sort can be established in a brainless dog, or in a dog or other animal deeply anesthetized with chloroform or ether, the mechanist's interpretation of the particular facts would be strongly supported and his general position greatly strengthened. But this has not been shown to be possible. The attempt to demonstrate this possibility should be the all-absorbing task of the behaviorist. But I do not know of any attempt at such demonstration; and all we know of the functions of the nervous system tends to make it appear very improbable that any such attempt can succeed" (p. 55).

All would agree perhaps that the experiment suggested would be worth attempting, but neither conditioned reflexes, behavioristic psychology, nor the mechanistic philosophy depends upon the results of such an investigation. The mechanist's position would also be strengthened if it could be shown that a dog's brain works mechanistically; it is not necessary to establish a conditioned reflex in a dog without a brain. The nature of a dog's cerebrum (associative neurons, etc.) accounts for the use which is made of it when conditioned reflexes are established. There is a more serious difficulty, however, and one which concerns Professor McDougall's belief that mechanistic principles do not apply to organic matter: namely, why would a dog die when his whole head is taken off? Neither nature which provides so bountifully nor the instincts which operate so beautifully are fit for the job when the whole head is removed. The burden of proof is not all on one side.

Although Professor McDougall seems to avoid the subject of child psychology, let us suppose, by way of illustration, that a baby is taught to fear a dog, and, furthermore, that another baby is taught to fear lightning, the conditioned reflex method of learning being used in both cases. The instincts do not determine *what* these two babies shall become afraid of. Our babies also do not seem to make much use of their free will in forming these connections and it is only in a limited sense that we can say that *they* do the learning. The *stimuli* would seem to be of some importance, but Professor McDougall objects to the concept of stimulus. A well-balanced psychology should consider not only the native tendencies of these babies but also the effects which are produced by the stimuli received. In making a psychological examination of these babies, we would naturally like to know something about their receptors, effectors, and connecting systems; but Professor McDougall considers that the stimulus-response psychology is not a proper subject for investigation.

It is very well to insist on anthropomorphic and purposive interpretations, if one chooses to do so, but we see no justification for supposing that either mechanistic philosophy or behavioristic psychology depend upon conditioned reflexes; this is probably more than the conditioned reflexes could bear. A more desirable method of criticizing conditioned reflexes would be to consider the experimental evidence itself, and attempt to point out the fallacies therein.

PUBLISHER'S NOTICE

The cost of printing has increased considerably since our present subscription rates came into effect in 1920. We have delayed making additional changes, in the hope that the peak was reached. On the contrary there has been a further rise in printing costs within the past few months. Our subscription rates no longer meet the cost of publication. We are therefore compelled, most reluctantly, to fix a new scale of rates for all our publications. The revised list appears on the second cover page. Subscribers will please note the advantageous combination rates offered.

The new prices will take effect July 1.

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